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Psychological Review

EDITED BY

HOWARD C. WARREN, PRINCETON UNIVERSITY

S. W. FERNBERGER, UNIV. OF PENNSYLVANIA (*J. of Exper. Psychol.*)
W. S. HUNTER, CLARK UNIVERSITY (*Index*)
HERBERT S. LANGFELD, PRINCETON UNIV. (*Monographs*)
E. S. ROBINSON, YALE UNIVERSITY (*Bulletin*)

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THE PSYCHOLOGICAL REVIEW

THE SCHOOLS OF PSYCHOLOGY[1]

BY STEVENSON SMITH

University of Washington

Mr. Hulsey Cason has gone to some trouble to discover for us what things in life are generally annoying. But I look in vain in Mr. Cason's list for that which to me is the supreme irritation, namely, being asked whether I am a behaviorist.

And I think I am right in allowing myself to become impatient over this question, and over the inquiry as well concerning my belief in structuralism. It is not that I wish to keep my opinions to myself. It is rather that, whether I say yes or no, my answer is certainly misinterpreted, for not to be a behaviorist may be taken to mean that I am a non-objective structuralist and, what is worse, to deny a belief in structuralism may lead to a charge of inconsistency were I ever to use in conversation the terms employed in the description of conscious states. This of course is the familiar 'fallacy of many questions,' and the absurdity is brought about, it would seem, by a disregard of the fact that the schools of psychology have their origin not in the laboratory but in the garden.

It would be unwarranted to deny to a psychologist the right to philosophize. But it is reasonable to ask that his experimental results, whose reliability is measured by a scientific method common to all schools, shall be kept separate from his metaphysical creed.

The experimental attack is not different in different schools. What the behaviorist has achieved is due to his being a good experimentalist and in spite of his philosophy.

[1] Address of the retiring president of the Western Psychological Association, University of Oregon, June 12, 1931.

When improved laboratory technique makes its appearance concurrently with school dogma the two are joined together in a loyal brotherhood that gives dogma an undeserved prestige. Whatever results survive from present-day configurationism will have a physiological, not an occult, explanation. The structuralism of the future will be independent of any philosophy but the philosophy of science, and its data will be inspected only for their reliability.

Varying degrees of belief may be felt in the sum of the propositions that constitute a philosophical system. But the credibility of an experimental finding is determined for all of us alike. The probability of its being true is known when we have eliminated constant errors and when we know its reliability. Thus, any dispute concerning the laws and the principles of psychology is due to violation of scientific method or to ignorance concerning that which can be discovered. The only variable, then, which legitimately distinguishes schools of psychology is philosophical belief, and, like all beliefs, this is a continuous variable.

The fallacy of types is familiar to everyone who has even a speaking acquaintance with statistical method. This fallacy consists in assuming the existence of a multi-modality in a distribution that has but a single mode. The four-leaf clover is a type, in the sense of a statistical species, but not so the introvert, the pyknic, the moron, the criminal, or even the psychologist.

We may create nominally a classification and, by means more akin to magic than to science, we may admit any individual to a class so created. The introvert is then an introvert because he is so listed in the records. The pyknic owes his inclusion in the group to the accident that fixed the arbitrary limits of the class. The moron is a diagnostic creation. The criminal is a creature of juristic mischance. The psychologist is distinguished from other men by an academic appointment or by membership in an association.

Classifications such as these are not altogether casual. There are similarities among most of the members of the class in addition to their bearing a common name. The significant

thing about these classes is, however, that, although the qualification for membership may be stated, the possession of the qualification by any individual can not be ascertained reliably and must be arbitrarily decided by judges or by test scales that are created for this purpose. Except for collusion any two judges will frequently disagree, and except for common elements two test scales will score an individual differently. For this reason the inclusion of an individual in a group is often more authoritative than natural.

If I am polydactylous I do not have to carry credentials in order that the world may admit my claim to membership in my class. I have merely to extend my hand and any child who can count to six will be convinced that I have a supernumerary digit. But if I lay claim to being a psychologist the world demands historical evidence that I have been so adjudged. Arbitrary though this classification may be, it is at least meaningful, as there is nearly always conclusive evidence that I am, or that I am not, in this class. But just what does it mean when a man lays claim to membership in a school of psychology?

In the field of belief, each individual is the accepted judge of his own classification. Each of us is a realist, a freetrader, a Marxian, an evolutionist, a configurationist, a behaviorist, or a structuralist, provided he says he is and provided he knows the propositions that constitute the verbal aspect of the opinions he professes.

In the disputed matters of taste a man's belief is justified and may be an important thing for others to know, but in the field of science, belief in one of a number of possible answers to an unsolved question is absurd. There are, of course, domains in science where conclusions are baffling, because of the inadequacy of data, to all who have so far attempted to derive a generalization. In such a case a scientist may rightly entertain a provisional guess as to the final outcome. But this can not justifiably be a conviction.

It may be contended that the schools of psychology disagree not on matters of observation but on questions of scientific method, and that these methodological questions

are unique to the field of psychology and so can not be settled in the court of general science. This may be true, but until such a finality is forced upon us we should seek a reconciliation of these differences.

There is probably no question concerning the validity of any proposed method in psychology the answer to which would not be obvious to all of us upon the application either of mathematics or of the qualitative rules that all science accepts. It would be impossible to prove this assertion here and it must remain a postulate of this paper. But if this is true there are no irreconcilable differences among the schools of psychology except in so far as they trespass beyond their field in a futile espousal of some philosophical doctrine that can never become a part of their science.

The essential question, then, and one which must be answered by anyone attempting to rid himself of the cultism of the schools, concerns the proper subject matter of psychology. The scientific treatment of this subject matter is, I think, a thing about which all schools might agree. The only philosophical bedfellow that psychological observation needs is scientific method. Not that scientific psychology may not be viewed and evaluated from every angle of metaphysical belief, but this comes later. Such an interpretation should not be used as a datum of psychology. No philosophical bias should be allowed to affect scientific technique.

In determining the proper subject matter for psychology our first step is the exclusion of material that can not be dealt with by experimental method. Such material, according to many objective psychologists, are the so-called states of consciousness. Shall these be thrown out of the laboratory? Certainly much published work in the field of introspection would never have been undertaken by an impeccable methodologist. Nevertheless, I believe that it can be shown even to the objective psychologist that these conscious states have not only a useful but an essential place in experimental procedure.

All psychologists, including self-styled behaviorists, act as their own subjects and observe their own conscious content

while working out their laboratory set-ups. If they were not to do this they would overlook important sources of error in their measurements. I think that every experimentalist will recognize as his own this method of identifying his reactions as well as of checking his laboratory conditions. But the behaviorist is likely to say that, adequately equipped with recording instruments, an experimenter has no need of any introspection.

The chief incongruity in this position is not to be found in the obvious fact that an adequate equipment of laboratory recording instruments is spacially impracticable. It lies in the fact that a very superior set of recording instruments is already present and available in the experimenter's own bodily structure.

The behaviorist, however, prefers his eyes to his interoceptors for the observation of human events. And where an unlimited choice of these sensory modalities is offered the behaviorist is wise to use his vision, for to certain forms of stimulation of the retinal area he is best conditioned to respond with space-time words. But the choice is not always offered, and often the only available means of observation is through some sensory field whose stimuli have not, through training, come to arouse space-time words. For all this, such words as they do arouse are not without their value. These words usually refer to experience more than to things. The hope of the objectivist is that every experience is an experience of something, and that this thing can become known to us more fully. It is the objective significance of an experience, not its intrinsic aspect, that has a place in objective science. To muse upon the blue of blueness may well get us nowhere. Likewise it would be futile to ponder, as such, the shape of the curve on the kymograph record. But blueness signifies many things, both in the structure of the outside world and in the structure of man, just as the kymograph record signifies something of the activity of the subject that is being observed.

The physical sciences assume an objective world, independent of any one person's experience. The practical function of science is to predict the future and to explain the past.

The assumption of a real objective world makes this task possible of accomplishment, and the meaning of the objective world is inherent in the term *reliability*.

The physical sciences do not use the language of the purposive aspect of experience. They do not speak of the love or hate of atoms, and they do not speak of objects as perceived. They speak of them merely as objects. The reason for this is that perception varies from man to man, but that the perceptions of many observers group themselves about a center of dispersion.

The language of science must be brief. Of two descriptions, the simpler is to be preferred. Although brevity alone cannot produce simplicity of description, it assists in producing it. The language of science must be clear. Its terms must refer to particular objects, classes or relationships. The use of terms without a definite meaning results in conclusions that have even less meaning. The application of vague generalizations leads to still further error. The language of science must be complete. It must be such that any observation may be classified in the same way by independent observers. When science lacks a term by which to classify a given observation, the observation will be variously described by different observers in language they have been forced to invent. Such a process leads to confusion.

Science is written in words, or symbols. Scientific description differs from ordinary conversation in that it does not include gestures or emotional expression, and its vocabulary does not include all the words in common use. It has no expletives. Its terms are few and, when the ideal is realized, well-defined. With these terms it attempts to describe, explain, and predict events.

Some of the terms of common speech refer to types of object and to their relations. Some refer none too definitely to man's behavior. Others refer to conscious states. It is only the first of these kinds of term that is used in the physical sciences. The biological and the social sciences employ terms that loosely describe behavior. Psychology is interested not only in physical events and in behavior, but is supposed to concern itself also with conscious states.

The sciences have long been conceived as constituting a hierarchy, with mechanics occupying the fundamental position and psychology the position furthest removed from simplicity. It is the yet unrealized hope of science to reduce all ateleological sciences to mechanics. By the reduction of a science we mean finding for its terms a combination of terms in a more fundamental science that are the equivalent of these. This has been to some extent accomplished in physics, where sound, light, heat, and electricity are stated in the dimensions of mechanics. Considerable progress has been made in the reduction of chemistry to physics, and in the reduction of physiology to chemistry. Although it may never be possible to so reduce all the ateleological sciences to terms of length and time, the attempt at reduction has resulted in a simplification that has made the world easier to understand.

It should be noted that when one science is reduced to another we do not do away with the terms of the more complex science. Many of these terms are still found convenient for common use. When heat is reduced to thermo-dynamics, we still talk of temperature. When we are speaking the language of mechanics, however, the term temperature is not only cumbersome and unnecessary, it is inadmissible.

If we find, in the more objective terms of psychology, correspondences for the terms of consciousness, we bring two fields of investigation into scientific relationship. If we can accomplish this task, we shall not deny consciousness, we shall merely relate it to physical science. For centuries the language of psychology has involved the use of both subjective and objective terms. The development of any science depends upon its selecting terms that can be shown to have a quantitative relation to each other. The terms of subjective psychology taken alone can have no quantitative relation to each other. The terms of objective psychology *can* be used to build up a quantitative science. Most psychological experiments are reported in objective language, and a growing tendency is found among psychologists to state the laws of their science without the use of subjective terms, even though they have used introspection in securing their results.

Any word begins to have meaning when it is spoken by two or more persons as a result of their observing similar objects or situations. Any word is itself a situation and gains meaning if it is responded to in the same way by two or more persons. As particular responses to a word become widespread, and as a word comes to be generally used in response to a particular situation, the word increases in meaning. A word has no meaning, however, if it is but a stimulus to another word or if it is given merely in response to another word. We find examples of meaningless jargon in books written by the organizers of certain cults. Many of the statements found in these books have not the dignity of being either true or false. Their absence of meaning, however, does not prevent their being used in a most predictable order by all the members of the cult. Nonsense words can be exchanged by two persons who have been trained in their use in a way that superficially resembles intelligent conversation. Even though, when I say 'dickery,' you may be expected to say 'dickery dock,' these words have no meaning because they have no objective reference and are cues to no behavior but verbal behavior. Such words are permitted only in Mother Goose jingles.

In so far as the language of introspection is *not* given meaning by its objective setting it can have no meaning, for the individual consciousness is never shared by two observers. In so far as the language of introspection *is* given meaning by its objective setting, such language refers to, and reveals, the objective order for other men than the verbalizing observer. Because of this there is much introspection that is significant, and indeed essential, for objective psychology. But introspective 'accounts' isolated from all objective reference are of no more interest to the objective psychologist than are such words as 'hi diddle diddle.' Consciousness without objective reference is private and no meaningful language can ever be used concerning it.

We like to assume that if two individuals were to have the same bodily states and the same environment, their subjective states would be the same. This, of course, is not self evident.

All we know is that the *behavior* of two individuals is more alike, the more alike they are in body and in environment. Their conversation about consciousness is objective and is made up of words all of which were learned in response to objective situations. If this were not true, no two men could mean the same thing when they use subjective language. In this very real sense all meaningful subjective language refers to objective situations. Shall we not, then, be content to describe the objective world, which is a point of reference, rather than attempt to describe a hypothetical community of conscious minds that we infer by analogy and of which we have no direct information? Any affirmative answer to this question must, I think, be qualified.

Because a part of the world, such as a chair, may be observed in the same way by all men, and because a part of the world, such as Mr. Jones' tonsils, may be observed by him in a way in which no other person can observe it, certain experiences of two observers must be forever different. The self observer knows something that is often lost when he attempts to communicate it to others in language that is untinged with subjectivity.

That all subjective experience has its correspondence in brain activity is assumed by most introspectionists. That all brain activity is made possible by sense-organ stimulation is orthodox theory in physiology. There must, then, be an objective situation that corresponds to any vague experience that captures the introspective interest. Many of these situations are unknown to us, but they are far from unknowable. Their identification is the task of experimental psychology, but this task would be impossible without the help of introspection. Introspection is of most value to objective psychology in the study of objects of experience that form a part of the observer's body.

A person's body is, for him, not only a part of the situation from which he can never escape, but also a part that in certain ways is known only to him whose body it is. There are certain corporial domains concerning which each of us has sensory cues that can be had by no one else. These cues often

cause us to talk in language that we classify as subjective. The subjective terms that the observer uses to describe his experience have, however, all been derived from the occasions when he and another person have been exposed to the same objective world and when the other person has described that objective world in these terms. This applies not only to the person who says, 'I have a sensation of red,' but it applies also to his testimony in the field of his more private experience. If he says, 'I have a stomach ache,' he is merely repeating what his mother told him, on certain occasions long ago, when she observed in him a certain kind of behavior. Two persons have never given the same name to their private experiences unless their respective experiences have had a common objective setting.

Words with subjective significance have been developed to enable a person to indicate to others a part of his environment that the others cannot readily share. All subjective terms refer eventually to objects. Although the observer of a subjective symptom may not know the object, he is like a baby who does not know that there are cannon in the world, but who hears a boom. Introspective terms usually refer to personal objects, such as the stomach. These terms do not describe the stomach as a whole, that is, they do not describe a stomach as known to us through all our sensory avenues. An introspective term may describe the object as known to us through but a single avenue of sense, such as a pain receptor. When we know the object through its stimulation of many receptors, we find it more useful to describe it in objective language.

Subjective terms may also be applied to objects that are equally observable by many observers. We may consider separately a certain part of our knowledge of a book; for example, that part which we get through a retinal sense organ, namely redness. If we permit light reflected from only a small portion of the book to stimulate the observer, he will not know that it is a book that he is looking at. It might be anything that is red.

Introspection deals with the experience of objects because

there is no other kind of experience. The introspectionist may not know the object the experience of which he is considing. When he cannot share with others the object of his experience, he may still be able to describe the experience in terms that are useful. Suppose the observer wishes to identify the internal bodily changes that follow a narrow escape from some sudden danger. This identification may be made possible only by an introspective account in terms that shock a behaviorist. Most of our implicit emotional responses would have gone forever unnoticed but for introspection. When the introspectionist, however, describes his experiences in terms that give us no clue to their objective origin, his words have no interest for objective science, and cannot be related to it.

Certainly not all subjective data are to be discredited, even though introspection with no objective reference may be unreliable. As a cue to objective findings, the observer's conscious state, not yet related to the objective world, may come to be so related. Indeed, this synthesis of experience into objective meaning is what takes place in the baby's developing into the man. Objective description can be verified. Subjective description, whose correspondence to the objective world can never be known, has no meaning. Language without objective correspondence is preeminently the language of deception. 'I love you,' 'I feel pity for the man,' are, taken alone, not facts. Such statements are social tools. To have scientific significance language must describe what is objectively observable by more than one person.

The glove I am wearing you do not experience tactually in the way that I do. Both of us may handle the outside but only I am feeling the inside of the glove. If I remove the glove you may thrust in your fingers with their sense organs and so share with me the inside of the object. But I have certain personal domains that you cannot share. My sense organs are permanently thrust into my own muscles and viscera and cannot be withdrawn in order that you may put your sense organs into their place, and so experience my bodily states. Could we effect such a transfer of sense organs you would feel my pain from a sprained ankle, my love for my

sweetheart, my nausea during a rough voyage, my hunger after fasting and my pleasure at a symphony. That you cannot observe these aspects of my body is because of the spacial distribution of your sense organs. The only receptors by whose aid you could make the observation are otherwise employed in a body of your own.

When you go to a physician you carry information with you of which the doctor would be forever ignorant but for your testimony. He feels, looks at, and listens to your body, but usually needs more facts than he can get in these ways before he is able to make a diagnosis. You, the patient, can feel, look at, and listen to your own body, and knowing medical technique you can secure in this way as much information as the physician can. In addition you can report certain facts concerning your malady of which the physician can have no first-hand knowledge. You have sense organs at the seat of the trouble and the doctor has none.

What the medical man can find out only by using the patient's sense organs he calls a subjective symptom. A stomach ache is such a subjective symptom. The patient does not say, 'I have a pyloric spasm.' We shall suppose that he does not know this. He is still in the introspective stage. He simply has an ache. After seeing his X-ray plate and having matters explained to him by the doctor he may progress to the point of describing his trouble objectively. Although his stomach in its painful aspect is still his private domain he may, upon a recurrence of his illness, tell the next physician that he has a spasm of the pylorus. In giving priority to the objective description he does not deny the existence of the stomach ache. He merely recognizes the greater practical value of the objective description. Such a choice of terms is not a new one to him. Even as a child he chose the objective description whenever he knew it. He always said, 'I have a splinter in my finger,' rather than, 'My finger hurts.'

An objective psychologist can not condemn introspection as long as he uses it as an aid to objective findings. Although a description of behavior and the objective world is always his

goal it is not always his starting point. He does not remain content with musing about conscious states or with inventing names for them. Only when he has found their objective antecedents does he emerge from revery and meet his obligation to scientific method.

The structuralist is forever relating his analysis of experience to his analysis of the objective order, and in so far as his findings are free from the invalidation of constant errors and achieve scientific reliability they become tools in the hands of the objectivist. In practice structuralists are objectivists, and objectivists, structuralists. The real difference is one of emphasis, and emphasis is a matter of rhetoric, not of science.

I began by admitting to a feeling of annoyance when asked to what psychological school I belong. As I see it, the reason for this annoyance is the question's implication that I take myself far too seriously; as seriously, in fact, as the questioner seems to take himself. If scientific method does not dictate my belief, as well as that of my questioner, I have small interest in seeking inclusion in a belief class whose members are elected by self acclamation.

[MS. received July 21, 1931]

SOLIPSISM IN PSYCHOLOGY

BY ALBERT P. WEISS

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Problem.—For those who teach courses in theoretical psychology, the philosophic problem of reality constantly arises in one form or another. The difficulties are just as acute for the behaviorist as they are for the mentalist. To say that 'that chair before me is *only* my chair reactions' (the behavioristic formulation) presents a difficulty of the same order as 'that chair before me is *only* a complex or unity of mental states in my mind' (the mentalistic formulation). To the student entering upon the theoretical aspects of psychology both formulations seem to be extravagant, not to say absurd.

From the point of view of psychology, the four fundamentally important theories of reality may be briefly described as follows:

Psychical Monism.—This point of view assumes that the universe is completely described in terms of sights, sounds, touches, temperatures, and the organic and vestibular sensations. All other complexes are derived from these simple elements, and these may be combined into perceptions, conations, affections, volitions, and purposes. However, most systems of psychical monism also postulate a physical system of sense organs, neurons, and effectors, although this is not essential.

Physical Monism.—This system assumes that the universe is the totality of the chemical and physical processes which are occurring and that the individual himself is only a locus in this continuum of physical and chemical processes.

Psychophysical Interaction.—This represents a relationship between elements of psychical monism and those of physical monism in such a way that the physical elements may produce psychical effects and that psychical effects may produce physical effects.

Psychophysical Parallelism.—This relationship assumes both psychical and physical monism, but instead of assuming a causal relationship there is merely correlation without causal interaction.

While from the philosophic point of view there are many definitions of solipsism, for psychology the definitions can be made relatively simple. Solipsism is a name for the fundamental nature of reality which is assumed to describe world structure and change. By fundamental reality we refer to the last stage of *verbal* analysis into objects or experiences such as form the synthetic complexes of units as electrons, protons, atoms, aggregates, relations, and sensations, images, feelings, mental states, *Gestalten*, conations, and volitions.

1. Thus in mental solipsism some complicated experience, such as perceiving an object, is analyzed into visual, auditory, tactual, organic, etc., sensations. The units of reality thus become the various simple sensations which are present in the perception of an object or the relation between objects. Since sensations seem to occur only within the body of the individual who has them, the sensations of other persons must be inferred, and they can only be inferred from the sensation which the first person has. From the fact that no two individuals can have each other's sensations it follows that the external world for every individual is an inference derived from his own experience.[1]

2. In physical solipsism we start with an object such as a stone and by analysis secure its weight, specific gravity, spatial dimensions, chemical composition, atomic structure, and (as far as we have gone) its electron and proton structure and relationships.

Roughly we can say that mental solipsism reduces the universe to visual, auditory, tactual, temperature, vestibular, and organic sensations plus pleasantness and unpleasantness. Physical solipsism reduces the universe to the simpler elements of the centimeter-gram-second system plus such relations as those implied under the physical, electrical, and mechanical relations, and ends with the properties of objects and relationships.

[1] I must leave the reader to form his own definition for the time being.

The first thing to be noted about these 'fundamental realities' is that they are the products of a highly sophisticated and trained individual and his environment. For the newborn infant there is no such thing as a fundamental reality unless we choose to make such an inference from an analysis of his behavior. The various combinations of biophysical elements and relations may vary from the complex biosocial relationships. Whether these biosocial relationships are also to be called solipsistic will depend upon what we wish the term solipsistic to mean. At present they are restricted to biomental relations or merely mental states.

The Language Difficulty.—The difficulty is a linguistic one. All languages have been developed on a non-solipsistic assumption in which an external reality is already accepted. We never express ourselves in solipsistic terms. For instance, when the non-solipsistic statement 'That chair before me is only my chair reactions' is cleared of the non-solipsistic implications, such terms as 'that chair before me,' 'only,' and 'my' must be eliminated, because they already imply an external world. The phrase 'that chair before me' implies that there is something in addition to the chair. Using a strictly solipsistic phraseology, the statement 'that chair before me' becomes simply 'chair reactions.' It would even be incorrect to say 'my chair reactions,' because the term 'my' implies a 'your' or a 'not-my.' There is no 'only' when there is nothing else with which to compare it. Performing these substitutions in our original non-solipsistic 'that chair before me' etc., we derive the solipsistic statement, 'Chair reactions are chair reactions.'

The objection to solipsism now becomes clear. It is practically and scientifically futile in the sense that the statement 'Chair reactions are chair reactions' is not a stimulus which modifies behavior, or produces better cooperation between individuals, or initiates a program of scientific investigation. No one has yet developed a solipsistic language, because it is useless.

The same holds for the mentalistic position. The non-solipsistic statement, 'That chair before me is only a complex

or unity of mental states in my mind,' when cleared of non-solipsistic terms becomes the solipsistic statement, 'The complex or unity of chair mental states is a complex of mental states.' Again, not a very illuminating statement. Certainly not the type of formulation from which one would expect a vigorous program of individual or social experimentation.

Another form in which the biosocial or behavioristic problem often arises is as follows: 'If the chair before me is only my chair reactions, then is there a chair when I am not reacting to it or when there is no one around to react to it?'

This statement again implies that a non-solipsistic universe (an external world) has already been adopted by the one who asks the question. Cleared of the non-solipsistic terms 'before me, my, I, it, no one,' we have: 'If the chair is only a configuration of mental states, then is there a configuration of mental states when there is no configuration of mental states?' Again, a sterile verbalization.

The Biological Basis of Solipsism.—The preceding linguistic analysis shows that a non-solipsistic assumption has been general for a long period of time. One may ask, how did it arise? The biological analysis may take the following form. The responses of the individual are limited to his own sensorimotor processes. This assumption was derived from generalizations which were already vaguely established in so far as primitive Man had already learned to respond to his own behavior history and to that of others. The discriminations (among many others) that he already had learned to make were as follows: there was a time (before birth) when he did not move as he does now, and a time will come when again he will not move (excluding theological considerations). Every person can name some approximate date in the past when he first began to respond. There are great individual differences. Some adults claim they can describe certain actions which they performed when three years old. However well such reports can or cannot be verified, there is for every individual a time limit, but for no individual does this extend backward to the day of his birth. There is always a beginning

which can be dated approximately and which starts some time after birth. Previous to this period there was nothing, and a hundred years later there will again be nothing (as far as any given individual is concerned). Between the beginning and the end, responses do occur, both simultaneously and successively, and the locus of these responses has been given various names such as I, the self, the personality, the individual, the ego, my sensorimotor system, my body, the organism, etc.

However, the movements of all individuals did not start and stop together. Sometimes he (the individual) was present at the birth of an infant (the beginning of movement in another); he learned to react to persons whose movements were coming to a close (dying individuals); the greatest number of his reactions were to individuals of his same social status. He even learned how to stop the behavior series of others (through warfare and fighting). He soon learned that other behavior life histories had occurred before his own, and would probably extend beyond him (his children). That is to say, taking his sensorimotor system into consideration, even very primitive man established the rough generalization of continuity, which is the modern non-solipsistic hypothesis of an external world.

Under a rigid, purely scientific description the only statement which is acceptable is one formulated to read somewhat as follows: Reactions [2] are occurring, there was a time in the past when they did not occur, and there are times now when they do not occur (under complete anesthesia or dreamless sleep). Such a formulation, while scientifically accurate, is

[2] The term 'reaction' need not cause serious difficulty. It is possible to define this term accurately in sensorimotor terms or to substitute for it the term 'movement,' which is completely described by its two properties, direction and rate, although the *measurement* of direction and rate presents greater difficulties than the *description* of the reactions in sensorimotor or social terms. Thus, the reaction of 'reaching for a pencil' (a social description) for most discussions is clearer than an enumeration of all the sensorimotor and anatomical components that are involved, and certainly much clearer than an enumeration of all the subatomic, atomic, molecular, and molar changes that are involved in the act. However, the difference in clearness is relative, depending upon the nature of the discourse, whether biosocial, biological, physico-chemical, or mathematical-physical.

also scientifically sterile, in that it does not present problems which may be experimentally investigated. The formulation is biosocially sterile, because in its linguistic form it is not an effective stimulus for social cooperation or interaction. To adopt a solipsistic system leaves nothing to be learned or investigated. Causal relationships vanish. There is only a temporally limited succession.

For scientific theory this biological side of solipsism is more important. It brings very clearly into the foreground the fact which scientists have failed to discriminate, that something like an electron-proton universe is a necessary assumption. Any species to survive (and this includes *Homo sapiens*) must make this assumption either implicitly or explicitly. Any individual who acted as if there were no external world or who responded as if all of his own actions originated in his own sensorimotor system (*i.e.* without external stimuli) would not survive in the struggle for existence. Nor would he live long enough to have offspring to inherit this method of behavior.[3] To state the situation in more extravagant terms, those individuals who do not inherit or acquire the *illusion* [4] of an external world vanish in the struggle for existence.

The observational fact is that there are sensorimotor changes during life and that these changes can be described or recorded by the individual himself as the totality of his responses or his behavior life history. When he stresses the fact that the sensorimotor effects are the only ones that are ever recorded or expressed, he is merely making a statement from which the assumption of an external world has been

[3] It is only necessary to point to the elaborate educational methods to teach the child to discriminate between his own body and his environment to show the biosocial importance of the assumption of an external world. The primitive man who avoided the tiger by simply closing his eyes has been eliminated.

[4] This statement does not imply that an illusion, in the mentalistic sense, can be inherited. The reason for using the term 'illusion' in this sense is to stress the *assumptional* character of the external world. It merely implies that an illusion which is so general that it is held by everyone is no longer an illusion, but a reality. The same is true for the term 'miracle.' When one person walks on water only a few times (and other individuals do not), we call it a miracle. However, if everyone were able to walk on water at any time, it would no longer be a miracle; it would be an established fact, although a rather troublesome one for the physicist.

excluded. In this sense the solipsistic statement, 'That chair is *only* the sum of my chair reactions,' is a scientific statement. Not a very valuable one, to be sure, since from it no definite program of experimentation can be developed. The biological statement of solipsism thus reduces to: 'Responses (changes) started at some moment in the past, and they are now following and paralleling each other.' The biological basis for a non-solipsistic assumption lies in the fact that any species that does not react as if an external world exists does not survive.

Mentalistic Solipsism.—From the mentalistic approach the solipsistic statement is of the same order, although it requires an additional assumption. It is first necessary to distinguish two types of mentalism. In one form such terms as consciousness, awareness, mind, volition, cognition, are used merely to include forms of sensorimotor or biological processes which are at present obscure or unknown but which with further investigation will turn out to be physiological and social processes. This form of mentalism is essentially biosocial, or behavioristic, in principle, and the solipsistic status is identical with that just discussed under the biological point of view.

The second type of mentalism assumes that there are mental states in addition to the physiological sensorimotor processes.[5] Here the solipsistic statement reduces to: 'At one time there were no mental states about a chair; at present mental states about a chair alternate with other mental states about other things.'

Other minds or other mental states need not be implied. However, the assumption that there are other minds and other mental states has been so firmly established that the discrimination of the discontinuous character of the individual's mental states from that of other individuals is always implicitly made. Hence the statement 'That chair is only a complex of mental states' sounds strange or unusual. It is, however, a statement of fact, although again it is not a par-

[5] This type of mentalism includes (1) mental or conscious states, (2) physiological or sensorimotor processes, and (3) an external world of stimuli which act upon the sensorimotor system.

ticularly useful statement from the standpoint of individual or group survival.

Solipsism in Physics.—Most of the support for the non-solipsistic assumption of an external world has been supplied by the science of physics. The fact that the hypothetical character of an external world has so long escaped separate scientific formulation [6] is due to the elaborate and convincing evidence which physical science has developed to demonstrate that it is *not* an assumption. It is out of place to go into details here. It would require the biosocial analysis of all the experiments and demonstrations which support the formulations of the mechanical principles involved in such statements as: the energy in the universe is a constant, the conservation of mass and motion, the sum total of the kinetic and potential energy of the universe is a constant, etc.[7]

For the psychologist the mechanical formulations can be replaced by the simpler non-quantified statement that every change is preceded by another change and is followed by another change. The term 'change,' while accurate enough in indicating the variability which occurs, is not so clear as the term 'motion' or 'movement,' which can be defined by the two properties of direction and rate. This brings the psychological assumption somewhat nearer to that of the physicist's conservation of energy. If we make this change, the mechanical formulation of the physicist, for the psychologist, becomes: 'Every movement is preceded by another movement.'

In this form the assumption does not require the one-to-one energy relationship which is explicit in the physical formula. This makes it possible, for instance, to affirm that a relatively weak movement (such as a verbal stimulus) may

[6] As a philosophical problem it dates back to Plato, at least.

[7] It is true that some of the physicists are beginning to question these formulations, but instead of discriminating them as the type of behavior that is classified under the category of verbal assumptions, they are going back to old psychological formulations in which such terms as soul, spirit, free-will, ideas, psychic energy, etc. are the necessary antecedents of human behavior. Science is not advanced by such speculations. On the biosocial side they are even harmful in that the physicists' *doubts* (not their experiments) are used to support reactionary theories of education and of social control from which human civilization has long been struggling to emancipate itself by substituting for them the methods of scientific experimentation and investigation.

be followed by movements which exceed the stimulus energy a millionfold (mob action, for instance). This does not imply that the one-to-one energy relationship of the physicist does not hold. The physicist would say that the psychologist has neglected such physical and chemical factors as amount of food eaten, energy stored in the body, effect of catalytic action as represented by endocrine secretions, the trigger character of the sensory process, etc. The psychologist might well agree with this but affirm that for his particular investigations (the biosocial interactions between individuals) the one-to-one energy relation is unimportant. For the physicist, on the other hand, the psychologist's problem (individual variations in behavior) is unimportant, because the physicist selects responses (pointer readings) in which the reactions from individual to individual have already become so uniform through social interaction and special training that the degree of variability may be neglected, and the effect of an observer on a physical experiment may be regarded as a constant in the mathematical sense. From the physical standpoint solipsism excludes functional [8] relations. Scientific activity is largely centered around causal relationships. If there is objection to the term 'causal,' the term 'functional' (again only in its mathematical or correlational sense) may be substituted. The solipsistic hypothesis gives only a principle of organization which is restricted to limited temporal succession. A functional hypothesis introduces a large number of relations a few of which are indicated by such terms as number, physical, chemical, biological, social, etc. It is these relations which furnish the program of experimentation, and generalization. It is for this reason that the psychologist, as a scientist, has

[8] The term 'functional' as used here refers to its mathematical implications as developed in the Theory of Functions. I wish specifically to limit the term to this use and exclude the dozens of quasi-explanatory ways in which the term is used by psychologists and sociologists. A simple illustration is that of the thermometer. The length of the mercury column is a function of the number of air molecules per cubic centimeter when surrounded by air. The longer the mercury column, the fewer the air molecules per cubic centimeter. It is not a reciprocal function, since changing the length of the mercury column (by applying ice or a lighted match to the bulb) does not change the number of air molecules per cubic centimeter, except to the extent that the ice or lighted match have actually changed the temperature of the whole room.

adopted the functional (mathematico-physical) hypothesis. This refers especially to those psychologists whose investigations are directed toward problems which relate the behavior of man to human civilization. Those psychologists who investigated only a restricted type of behavior (the introspections or immediate experience of adults) did not require a functional hypothesis, although they did adopt one (psychophysical functions).

The Biosocial Analysis of the Development of the Functional Hypothesis.—When the psychologist accepts the formulation that change is continuous beyond his own sensorimotor system, he finds support for this in that within his own system of reactions he also finds continuity or sequence. That is, when he is reacting to the chair visually (seeing it at a certain distance), these visual reactions may be followed or accompanied by kinesthetic reactions (walking toward the chair), these in turn by tactual reactions (touching the chair), and these in turn by changes in visual, auditory, kinesthetic, and tactual reactions (sitting down in the chair). Since this series is relatively uniform for the individual (it may be repeated again and again), the statement that there is an external chair refers only to the fact that the individual has discriminated that the series is a uniform series whenever it is repeated and that there is no limit (as long as his sensorimotor equipment remains approximately the same) to the number of times the series *may be* repeated. There is nothing involved in this except the fact that when a response or a series of responses has occurred say a thousand times, it may occur once more, or twice more, or continually throughout the life of the individual. This does not imply that there is a logical inference or a generalization. It merely implies that the individual (with respect to this particular chair reaction) will continue to react as he has been reacting. From the biosocial standpoint, then, the non-solipsistic (functional) hypothesis rests upon the following assumption: Movements have occurred before the individual's movements began, they are continuing during the life of the individual, and they will probably continue after his death. The acceptance of this

assumption justifies the statement that the chair before me is independent of my reactions to it.

When the question arises, 'If the chair before me is only my chair reactions, then is there a chair when I am not reacting to it or when there is no one around to react to it?' so far as the writer is concerned he would say, 'Yes, there is a chair even when no one is reacting to it.' This statement is justified by acceptance of the physicist's view of an external universe of electrons, protons, atoms, molecules, etc. The degree of conviction as to the existence of an objective chair independently of one's own sensorimotor system is proportional to the extent to which he accepts the demonstrations of the physicist's proposition of the conservation of energy or the proposition that change is continuous. The trouble seems to lie in not making a discrimination which has become the fundamental assumption of science and of human behavior. Physical science assumes that the universe is made up of simple elements (electrons and protons at present) and that *an individual* is one out of many electron-proton aggregates. This is of course an assumption. That is to say, it is a verbal stimulus which in its visual or auditory form may modify human behavior. It is classified as an assumption because at the present time electrons and protons are not reacted to with the degree of uniformity from individual to individual (limited to physicists in this case) that may be established in reactions to chairs, chemical compounds, inches, seconds, pounds, etc. The problem of the psychologist is that of determining how this assumption of an external world arises in the infant or child. There does not seem to be any justification for believing that the newborn infant has any assumption,—either a solipsistic one or that of an external world. Yet he acquires the external-world assumption as he grows older. How he acquires it is an ontogenetic, phylogenetic, and a biosocial problem. It can, however, be made the basis of an experimental program, which has been regarded as impossible in the past.

Summary.—Present languages have been developed on a non-solipsistic assumption in which an external reality has

already been adopted. All of our words imply contrast or opposition, and presuppose an external world in addition to the mechanism which makes up the individual himself. When language is given a purely solipsistic form, the problems of solipsism vanish, and all statements about reality become tautological and scientifically futile.

The biological basis for a non-solipsistic assumption lies in the fact that an organism which reacts as if its own mechanism were the fundamental reality is unfitted for survival and soon vanishes in the struggle for existence.

A mentalistic solipsism is inadequate because of the great variety in the reactions of organisms, and because there is no different possibility of classification without assuming a physical basis. The possibility of a physical solipsism has never been realized until very recently. In fact the activity of the physicists has been primarily of the type to demonstrate that there is an external world in addition to the responses of the individual, and in all of the natural sciences this assumption has been accepted. It can be said that the aim has been to refute solipsism, although some of the modern physicists are beginning to question the validity of this assumption. However, in doing this they are reverting to psychological and philosophical systems which no longer represent the modern point of view of either philosophy or psychology. They neglect entirely the part which social cooperation has played in the development of our conceptions of a fundamental reality, adopting in place of this the earlier religious and mystical conceptions which formed an important educational unit during their training.

In the biosocial analysis of the functional hypothesis the psychologist accepts the [assumed] relation that change is continuous beyond his own sensorimotor system. Within his own system of reactions he finds continuity, simultaneity, and a definite origin and a very probable end of his own reactions. He definitely regards all conceptions of fundamental realities as products of the linguistic interaction between individuals, but in this linguistic interaction he finds only the elements of physics and chemistry, in which language repre-

sents a form of stimulation which has both ontogenetic and phylogenetic origins. Such fundamental realities as electrons and protons, atoms, molecules, and organisms are social products of interaction. The same is true for the realities included under sensations, images, affections, and those included under religion. None of these can be regarded as being present at birth. The development of any particular type of fundamental reality is the product of an environment and the ontogenetic and phylogenetic history of the organism.

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GOAL ATTRACTION AND DIRECTING IDEAS CONCEIVED AS HABIT PHENOMENA

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I

When an animal long without food is first placed in a maze or other problem situation it will usually, after a momentary pause, move about vigorously but more or less at random. These seeking movements are understood without great difficulty as the result of the combination of (*A*) the changing sensory stimulation emanating from the environment as the animal moves from place to place; (*B*) the changing proprioceptive stimulation resulting from the immediately preceding movements of the animal itself; and (*C*) a sensibly *non*-changing dynamic internal core of stimulation emanating from the continually recurring hunger cramps of the digestive tract. The changing stimulus components account to a considerable extent for the variability characteristic of the trial-and-error seeking behavior, the particular acts taking place at each instant being determined by the nature of the habituation tendencies set up in the previous history of the organism. The unchanging stimulus component, on the other hand, gives the various action segments of the behavior flux its characteristic unity by tending largely to limit the behavior to acts which in the past have been associated with the securing of food and which are therefore more likely than pure chance to result in securing it again. And when, at length, the food is found and eaten and the digestive crampings have ceased, the food-seeking behavior naturally comes to an end because its stimulus motivation has ended. Such, in brief, is the account of trial-and-error behavior conceived as motivated primarily by physiological drive.

But this is by no means the whole story. While probably

accurate enough so far as it goes, the above account presents but the first part of the learning process. Experimenters report that once the animal has found food at the end of the maze a few times, his behavior undergoes a striking qualitative change. The new behavior is often characterized as appearing to be more 'purposeful' than at the beginning; he acts, as Gengerelli remarks, as if he were 'going somewhere.' Beneath this rather vague characterization there may be discerned certain fairly concrete and definite behavior tendencies, the most notable of which involve anticipatory movements. When an animal is approaching his goal (the food box) he is apt progressively to speed up his pace. Another significant observation is that when an animal is approaching a familiar 90° turn in a maze, he quite generally begins his turning movements some time before he reaches the corner. Perhaps most significant of all is the phenomenon observed not only in all sorts of vertebrates but in young children and naive adults as well, that as a food goal is neared the organism tends to make mouth movements of a masticatory nature. It is probably not without significance that such movements are particularly prominent in cases where the sequence of acts leading to the goal is, for some reason, interrupted.

To casual observation such acts as the premature or anticipatory movements just cited are likely to appear as interesting symptoms of an obscure psychic tendency but in themselves to be of no functional value whatever to the organism. If this were the case they would, of course, be positively detrimental, since they would involve a wasteful expenditure of energy. In direct contrast to this view, it is the purpose of the present paper to elaborate the hypothesis that anticipatory goal reactions, as distinct from organic drives, play an indispensable rôle in the evolution of certain of the more complex forms of mammalian adaptive behavior and that the understanding of this rôle will render explicable on a purely naturalistic and physical basis the profoundly significant influence of rewards, goals, and guiding ideas upon behavior sequences.

II

Let it be assumed that a relatively isolated inorganic world flux takes place in time. Characteristic phases of the world sequence, separated from each other by but a few seconds each, are represented by S_1, S_2, S_3, etc., as they appear in Fig. 1. In the neighborhood of this world sequence is a

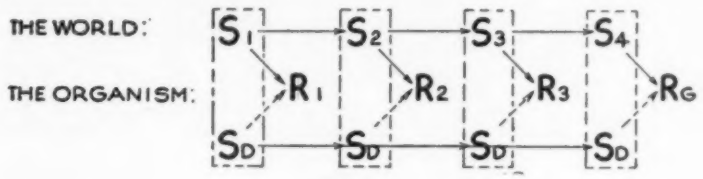

FIG. 1.

sensitive redintegrative organism provided with distance receptors and so constituted as to respond characteristically to the several phases of the world flux with a parallel behavior flux. Phases of the response flux corresponding to the world-stimulus flux are represented in Fig. 1 by R_1, R_2, etc., the final or goal reaction being indicated by R_G. Let it be assumed, further, that within the organism there is a source, such as hunger, which produces the continually recurring stimulation represented in Fig. 1 by S_D. Now, according to the principle of redintegration, all the components of a stimulus complex which may be impinging on the sensorium at or near the time that a response is evoked tend themselves independently to acquire the capacity to evoke substantially the same response. The stimulus complexes in Fig. 1 which fall under this principle are each enclosed within a dotted rectangle. It may be seen from an examination of the diagram that S_D, owing to the fact that it persists throughout the entire behavior sequence, will acquire a tendency to the evocation of R_1, R_2, R_3, and R_G, *i.e.*, to the evocation at any moment of *every* part of the reaction sequence. These newly acquired excitatory tendencies are indicated in the diagram by dotted arrows.

To amplify this part of the picture, there must be added the fact that each act, as R_1, gives rise to a proprioceptive stimulus, s_1. These proprioceptive stimuli are added to the

diagram in Fig. 2. Through the operation of redintegration, they likewise tend to acquire the capacity to evoke the reactions immediately following them. This second group of

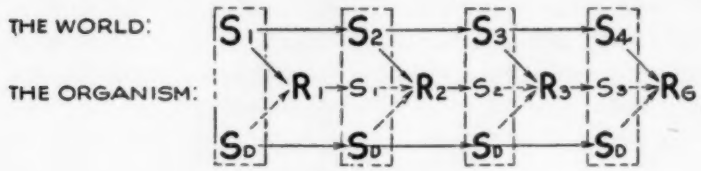

FIG. 2.

newly acquired excitatory tendencies is also represented by dotted arrows.

III

It has been shown elsewhere [1] that under certain conditions, notably when the behavior sequence is made up of symbolic or pure-stimulus acts, the multiple excitatory tendency of the persisting stimulus, S_D, may evoke the final or goal reaction of the series almost or quite at the outset of the movement, thus dropping out the useless and actually wasteful intervening acts formerly leading to the goal. But in cases where the intervening acts are mainly instrumental in nature, as is obviously the case with the locomotor activities involved in maze running, it is physically impossible to drop out any of the acts involved in traversing the *true* pathway and at the same time reach the goal.[2] With the maze remaining con-

[1] C. L. Hull, Knowledge and purpose as habit mechanisms, PSYCHOL. REV., 1930, 37, 511–525.

It should be noted that the mechanism of short circuiting behavior sequences there described is not adequate, as it stands, to explain the dropping of blind-alley entrances in maze learning. Space is here lacking for the elaboration of the particular mechanism involved in that specialized form of behavior sequence. Contrary to what seems to have been assumed by some, maze learning, instead of being a relatively simple process, is in reality one of great complexity. See C. L. Hull, The goal gradient hypothesis and maze learning, This journal, (In press).

[2] Perhaps this difference in short circuiting of pure-stimulus acts as contrasted with instrumental acts is their most revealing distinction. Pure-stimulus acts are defined as acts whose sole function is to evoke other acts through the proprioceptive stimuli which they give rise to. They thus conform in a physical sense to the concept of symbolism though the entire process may be confined to a single organism in contrast to communicational symbolism which involves the acts of one organism serving as stimuli to another. The pure stimulus acts considered in the present paper are entirely of the first or individual type.

stant, the space between the starting point and the goal must, somehow, be gotten over if the food is to be obtained. Consequently, if the anticipatory invasion by the goal reaction of the instrumental behavior sequence normally leading to the goal should result in the interruption of the sequence, the actual goal will never be reached and the episode will be biologically abortive. Such an interruption will inevitably take place either when (*A*) the invading goal reaction is of such a nature that it cannot be performed by the organism at the same time as the antecedent instrumental acts leading up to it, or (*B*) when the execution of the goal reaction results in the removal of the source of the physiological drive stimulus (S_D).

In view of the ever-present potentiality of a strong drive stimulus for producing anticipatory invasions of behavior sequences by goal reactions, it should not occasion surprise if these invasions should occasionally take place even in genuinely instrumental sequences. Those common wish-fulfilling delusions so characteristic of certain forms of dementia precox are probably cases where the miscarriage of this mechanism has led to its natural maladaptive issue. Indeed, all wish fulfillments appear to be of this nature, which doubtless explains their bad repute among psychopathologists. The sexual orgasm which takes place during the ordinary erotic dream is evidently of the same nature. Extreme *ejaculatio precox* in anticipation of the sex relationship is a still clearer example of the abortive results of the goal reaction being displaced forward in time, producing the typical biologically disastrous result of preventing the completion of a reproductive cycle.

That this abortive anticipatory invasion of the antecedent instrumental sequence by the goal reaction does not take place with as great frequency as does the biologically valuable short-circuiting of pure-stimulus-act sequences raises an important theoretical question. A plausible explanation of this difference is found in the nature of the stimuli complexes operative in the two cases. Except for remote excitatory tendencies, which are here neglected in the interest of simplicity of exposition, the typical stimulus complex of the in-

strumental sequence leading to a goal is shown in Fig. 2. Consider, for example, the stimulus complex immediately preceding R_2. It consists of the external stimulus S_2, the proprioceptive stimulus, s_1, arising from the preceding activity, and the persisting or drive stimulus, S_D. The typical symbolic series, on the other hand, being ordinarily an internal process, characteristically lacks in its stimulus complex the external factor, S_1, at least as a dynamic and coercive component. The significance of this stimulus difference becomes apparent when it is observed that the S_2- and s_1-components operate in the direction of a simple and stable chain-reaction tendency whereas the S_D, in addition to a chaining tendency, may have at the same time a very strong tendency to evoke other reactions, and especially the goal reaction. The relative potentialities of the two stimulus systems may be seen very readily by an inspection of Fig. 3, where arbitrary nu-

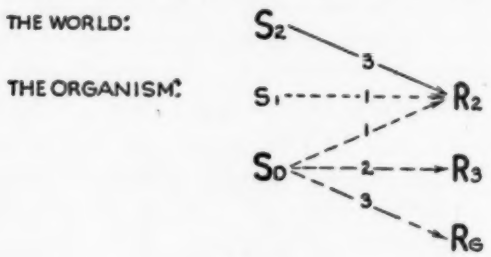

FIG. 3.

merical values have been assigned to the several excitatory tendencies. The simple summation of the potentialities of the several excitatory tendencies of the instrumental stimulus complex, $S_2\ s_1\ S_D$, yields the following values:

$$R_2 = 5$$
$$R_3 = 2$$
$$R_6 = 3$$

Here R_2 is definitely prepotent and the original sequence will not be interrupted. Clearly the chaining tendency is greatly stabilized by the presence of S_2. A similar casting up of the

action potentialities when S_2 is neutral or functionally absent (as in symbolic sequences) shows the following:

$$R_2 = 2$$
$$R_3 = 2$$
$$R_G = 3$$

In this case, R_G becomes prepotent. Here the chaining tendency is very obviously threatened with disruption as the result of R_G following immediately after R_1 with the consequent dropping out of R_2 and R_3.

From the foregoing it is evident that in genuinely instrumental sequences the goal reaction is not likely to be displaced backward in the behavior sequence if it is of such a nature as seriously to compete with the latter for control of the final common path. The powerful influence of the external component of the stimulus complex (S_2) ought normally to prevent this. But in case the goal reaction does not, either as a whole or in part, compete with the instrumental sequence, there seems to be no reason why the former should not undergo such displacement. In that case, the anticipatory goal reaction would not disrupt the instrumental sequence, but the two would take place concurrently. Presumably, however, goal reactions spread rather widely and ordinarily involve a large part of the organism. A wide involvement of the organism in the goal act would naturally tend strongly to interfere with any other complex concurrent activities with which it might be associated. Thus the appearance of complete goal reactions simultaneously with what previously were their antecedents should be rare.

There remains the more likely alternative that a split-off portion of the goal reaction which chances not to be in conflict with the antecedent instrumental series may be so displaced. Unfortunately, little is known experimentally of the dynamics of this fascinating possibility. Observation, however, supporting theoretical expectation, seems to indicate that anticipatory goal reactions appearing in the midst of normally antecedent instrumental act sequences are generally incomplete, fractional, imperfect, and feeble. Fortunately, with

pure-stimulus acts, weakness within limits is of no disadvantage.[3]

Moreover, anticipatory goal reactions appear to manifest themselves with special frequency, vigor and completeness on occasions when, for any reason, the smooth flow of the instrumental sequence has been interrupted. This, again, is in harmony with theoretical expectation, since at such times there would presumably be less competition for the final common path. As an illustration of this there may be mentioned a recent observation made in a Boston restaurant. A man and woman were leisurely eating their dinner. Sitting bolt upright at the table on a third chair was a handsome bull terrier. Throughout the meal the dog watched his master and mistress consuming the tempting morsels without himself making the slightest overt instrumental act leading to the obvious goal. Moreover, all overt anticipatory goal reactions (such as masticatory movements) appeared also to have been inhibited, quite in accordance with the best New England traditions. After a time, however, an implicit fractional component of the anticipatory goal reaction manifested itself; a long, thick thread of saliva was observed hanging from each corner of the dog's mouth. The carefully studied salivation of Pavlov's dogs evidently also represents anticipatory goal or terminal reactions. The less inhibited Russian dogs executed gross mouth movements such as vigorous licking of the lips as well.[4]

Despite the superficial appearance of a lack of physiological conflict between two such processes as locomotion and the goal activity of mastication, general observation leads rather strongly to the expectation that there would be considerable interference even in such cases. Carefully controlled experiments will probably show, for example, that salivation is more active when the normally antecedent instrumental acts are temporarily interrupted than when they are proceeding in full vigor. If this principle can be established experimentally it will have special theoretical significance.

[3] C. L. Hull, Knowledge and purpose as habit mechanisms, PSYCHOL. REV., 1930, 37, p. 515.

[4] I. P. Pavlov, Conditioned reflexes, p. 22.

IV

Having the phenomenon of the anticipatory goal reaction clearly before us, we may proceed to the consideration of some of its functional potentialities. Let us assume that in the dynamic situation represented in the diagram of Fig. 3, one portion of the goal reaction is not in conflict (competition) with its antecedent reactions, whereas the remainder is so. For convenience we may designate this non-conflicting component as r_G and the conflicting component as R'_G. It is assumed that as learning proceeds, S_D gets conditioned to the several phases of the reaction sequence and with an intensity roughly proportional to the proximity of each to the goal, the goal reaction itself possessing the most strongly conditioned excitatory tendency of all.[5] It is assumed, further, that this tendency, at least occasionally, will be sufficiently strong to evoke a weak r_G-reaction even at the outset of the series. This movement of the fractional goal reaction to the beginning of the behavior sequence together with its subsequent persistence throughout the cycle is indicated diagrammatically in Fig. 4. The persistence of r_G is due to the parallel persistence of S_D which continuously evokes it.

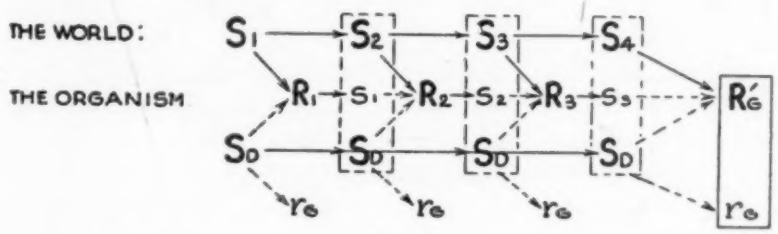

FIG. 4.

Like all other movements, r_G causes characteristic proprioceptive stimulations to arise from the muscles involved. This complex of stimulation flowing from r_G may be represented conveniently by s_G (Fig. 5). It is obvious that since r_G persists throughout the behavior sequence, s_G must also do so.

[5] C. L. Hull, Knowledge and purpose as habit mechanisms, PSYCHOL. REV., 1930, 37, p. 521. This as yet hypothetical drive stimulus excitatory gradient will be taken up in some detail in a forthcoming paper. A rather numerous and varied assortment of experimental observations substantiates the hypothesis.

It thus comes about that our dynamic situation is possessed of two persisting stimuli, S_D and s_G. Some of the potentialities of the drive stimulus (S_D), have been elaborated elsewhere.[6] The second persisting stimulus (s_G), by way of con-

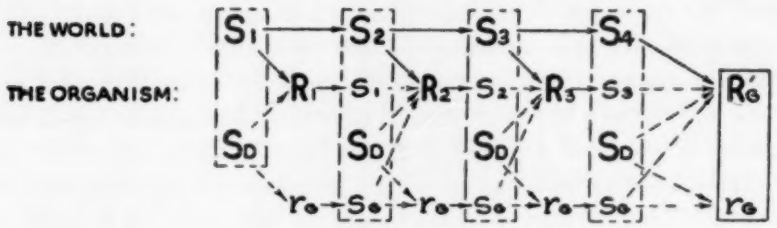

FIG. 5.

trast, will be called the *goal stimulus*.

It is at once apparent from an examination of Fig. 5 that, just as in the case of the drive stimulus, the principle of redintegration ought to set up excitatory tendencies from s_G to every reaction of the behavior sequence including, apparently, tendencies to both components of the goal reaction, the one to r_G being circular. The multiple excitatory tendencies in question are represented diagrammatically in Fig. 6.

FIG. 6.

Despite the very significant similarity of the goal stimulus to the drive stimulus there are equally significant differences which need carefully to be noted. In the first place, the two stimuli differ radically in their source, or origin. The drive stimulus, in the typical case of hunger already before us, evidently has its origin in the physico-chemical processes involved

[6] C. L. Hull, Knowledge and purpose as habit mechanisms, PSYCHOL. REV., 1930, 37, p. 519. In that article the drive stimulus, here represented by S_D, was represented by S_p. The present notation is believed to be the more appropriate.

in nutrition. The goal stimulus, on the other hand, is dependent, in the main at least, upon the existence of the drive stimulus and the conditioning of it to the goal reaction. The drive stimulus thus has an essentially non-redintegrative origin, whereas the goal stimulus is preeminently a redintegrative phenomenon. The drive stimulus is not likely to be greatly disturbed by either the presence or absence of the goal stimulus, but anything which terminates the drive stimulus will automatically bring the goal stimulus to an end.[7]

A second significant difference between the drive stimulus and the goal stimulus arises from the fact that from a single drive stimulus there may evolve many tolerably distinct goals. A rat, for example, will, when hungry, eat many different kinds of food. The eating of each kind of food may become a different goal with the goal reaction in each case presumably in some sense distinct. Moreover, the same kind of food may constitute the goal in many different mazes. It is evident that this possibility of a multiplicity of goal stimuli for each drive stimulus has important dynamic potentialities, especially in view of the small number of drives as contrasted with the immense variety of mammalian goals.

V

With the contrasted concepts of the drive stimulus and the goal stimulus well before us, we may now turn to the consideration of the rôles played by them in certain typical situations which are known rather well on the basis of specific experiment. The first situation to be studied is the well known one where an animal which has learned a maze with a reward of food consistently given, for a number of trials receives no reward of any kind. Under such circumstances the maze habit progressively disintegrates. Our interest is concerned with the mode of this disintegration. There is, of course, nothing surprising in the fact that a food reaction should suffer experimental extinction upon consistent failure of rein-

[7] This neglects the possibility that once S_D has brought r_G into the antecedent reaction sequence, stimuli there resident may acquire a tendency to evoke r_G themselves and thus bring into existence s_G independently of S_D. An example of such a possibility would be the excitatory tendency represented by $s_1 \rightarrow r_G$.

forcement. Nothing is more common in the conditioned reflex literature.[8] But this, as it stands, would seemingly account for nothing but the inhibition of the act immediately preceding the failure of the customary reward. The particular question which primarily interests us here is: Why does the habit sequence disintegrate as a whole? Why should an event (failure to receive food) taking place at the conclusion of a long series of reactions produce a disintegration of those habit segments most remote from the active origin of disintegration, quite as promptly and completely as of the segment (goal reaction) immediately adjacent? In terms of our diagrams (Figs. 4 and 5) why should an event taking place at S_4 produce a disintegration of habit organization at S_1?

The first hypothesis which appears to offer anything in the way of explaining this extensive disintegration is that the drive stimulus might acquire generalized inhibitory properties. Specifically, once the drive stimulus (S_D) has suffered experimental extinction as regards its goal reaction, this inhibition might conceivably spread to every other reaction into which S_D enters as a stimulus. But since S_D is present throughout the series the tendency to inhibition would thus spread from the termination of the sequence at once throughout its whole length and might, therefore, produce the generalized disintegration known to exist.[9]

As yet our knowledge of secondary inhibitory tendencies is too meager for us to express an opinion with any confidence as to whether a spread of inhibition would be brought about by the mechanism sketched above, and, if so, whether the tendency would be strong enough to produce the disintegration just referred to. The following consideration makes it seem doubtful: If the inhibitory tendency to reactions emanating from S_D were strong enough to over-ride the chaining excita-

[8] It is here tacitly assumed as a useful working hypothesis that principles found to be operative in conditioned reflexes are also operating in maze learning. This paper may be regarded as essentially an exploration of what might be expected in a complex learning situation in case this were true. Once the implications have been drawn, the way will be open for the setting up of critical experiments which may clarify the present unfortunate uncertainty concerning the relationship between the two phenomena.

[9] I. P. Pavlov, Conditioned reflexes, pp. 48, ff.

tory tendencies holding the behavior segments together in the series in which the frustration took place, it should also be strong enough to produce a similar disintegrative tendency in every other sequence which this drive (S_D) enters as a stimulus. This means that if the organism were consistently disappointed in finding food in one accustomed place until it would no longer seek it in that location there would also result a similar paralysis of all attempts to seek food in any place at all, at least until the inhibitory tendency should have subsided. And in that event the organism would be about as likely to seek food in the place previously proven disappointing as in some alternative place which has consistently yielded food. Since these deductions are obviously contrary to fact, at least in this extreme form, [10] we must seek some further or supplementary mechanism for the explanation of habit disintegration which results from withdrawal of reward.

A second mechanism which appears with somewhat greater probability as mediating the generalized disintegration of maze habit organization at the failure of the reward is the *goal stimulus*. We have already pointed out in connection with the discussion of the rôle of the drive stimulus that the goal reaction (r_G) would naturally be the one to be eliminated at once as the result of failure of reinforcement by food. But when r_G is eliminated, there is automatically eliminated s_G also, together with the influence of its multiple excitatory tendencies (Fig. 5). And, since s_G is a stimulus component of all the reactions of the sequence, they will all be correspondingly weakened by its absence, thus accounting for the spread of the effects of the failure of reward so effectively throughout the entire series.

The extent to which this weakening takes place will de-

[10] The *a priori* probability that the inhibition resulting from failure of reward in one situation should inhibit to a certain extent other tendencies to action from the same drive stimulus, at least for a time, is sufficient to warrant the setting up of a carefully controlled experiment to determine whether or not it exists. If, for example, a rat has learned two distinct mazes to a known and equal degree and then the habit of running maze No. 1 is disintegrated by failure of reward, the rat should show a perceptible tendency to disintegration of the second maze habit when comparison is made with a suitable control. A fundamental principle of considerable importance may thus emerge.

pend to a considerable degree upon whether the stimuli throughout the action sequence are in the state of mere unorganized complexes or whether they are organized more or less perfectly into stimulus patterns. In the former case any stimulus component, by dropping out of the complex, will withdraw from its joint excitatory tendency no more than what it would exert if acting alone. But in case the stimuli have become organized into patterns the dropping out of any customary component may result in the profound weakening of the joint excitatory tendency and even its complete abolition.[11]

Our main objective, however, is to emphasize a radical and significant difference between the habit mechanisms of drive stimulus and goal reaction stimulus respectively. We have already seen that a strictly generalized inhibitory tendency from S_D alone, if such exists, would spread alike to all reaction sequences which are mediated by this drive and would not permit of the differentiation of a successful series from an unsuccessful one. This would present an impossible biological dilemma. The goal stimulus supplies the key to the situation. Since r_G is peculiar to the one sequence of which it is the terminal action, the effect of its inhibition will weaken its own series alone. This will leave all other series, even when motivated by the same drive, essentially intact except possibly for weak inhibitory radiation effects. Thus an animal disappointed in finding food at one place, will no longer seek it there but will, nevertheless, proceed to seek it elsewhere. The disintegration of maze habits under the influence of withdrawal of reward thus serves nicely to illustrate both the inadequacy of the drive stimulus as a sole explanatory concept and the distinct explanatory possibilities of the goal-stimulus concept.

VI

The concept of anticipatory or premature goal reactions appearing in fragmentary form concurrently with acts origi-

[11] Presumably habits range everywhere between these two extremes of degree of patterning and any given stimulus may be in a constant state of flux in this respect. The experimental determination of the factors governing this fundamental tendency offers a rich field for systematic exploration.

nally antedating them presents a very striking similarity to what has long been known as ideo-motor action. Some concrete cases of what is meant by this latter term may assist in the grasping of this point. A familiar example of ideo-motor action is found in the pressure of the inexperienced billiard player against the edge of the table while observing the movement of a ball which he has just activated, particularly when the ball threatens not quite to reach the mark at which he has aimed. A somewhat similar example is seen in the common tendency of people when watching a football game to lean and even push in the direction that they wish the play to move.

To most psychologists in the past, ideo-motor action has been regarded as no more than an interesting curiosity—a kind of abortive activity having no functional value in itself but serving, perhaps in large part by virtue of its very maladaptive nature, to indicate strikingly the existence of a tendency which might be really adaptive under other circumstances.[12] From the point of view of instrumental behavior the maladaptive nature of most ideo-motor acts is evident. Nothing could well be conceived as more stupid and less calculated to influence the course of a billiard ball than a gentle pressure on the side of the billiard table. Few things could be imagined which would be more futile as regards the outcome of a football play than the sympathetic movements on the part of the spectators. In this, ideo-motor action presents an exact parallel to implicit anticipatory goal reactions such as the licking of the lips and the salivation of the dogs of the conditioned reflex experiments. Indeed if we consider that the pressure delivered to the side of the billiard table really would function on the ball if the hand had traversed the distance from the edge of the table to the position of

[12] This seems to have been true even of William James, despite the fact that he regarded ideo-motor action as intimately related to will. "Wherever movement follows *unhesitatingly and immediately* the notion of it in the mind, we have ideo-motor action. . . We think the act and it is done. . . Dr. Carpenter, who first used, I believe, the name of ideo-motor action, placed it, if I mistake not, among the curiosities of our mental life. The truth is that it is no curiosity but simply a normal process stripped of disguise." (William James, Principles of psychology, 1908, Vol. II., p. 522.)

the ball, and that the movements of the spectator of a football game really would affect the outcome of the play if he had traversed the distance separating him from the players, these movements appear quite literally as anticipatory goal reactions. As instrumental acts they are, and must always be, abortive, maladaptive, wasteful and stupid.

It by no means follows from the foregoing, however, that ideo-motor action is really maladaptive. On the contrary the view is here put forward that ideo-motor reactions and anticipatory goal reactions in general are really guiding and directing pure-stimulus acts and as such perform the enormously important functions ordinarily attributed to ideas. Considered merely as acts they are negligible; as pure-stimulus acts and sources of stimuli to control other action, they at once take on the very greatest importance. While indubitably physical they occupy at the same time the very citadel of the mental. The classical view was that a non-physical idea of an act preceded the act and somehow commanded the energy to evoke it, such act in consequence being called ideo-motor. In contrast to that view, the hypothesis here put forward is (1) that ideo-motor acts are in reality anticipatory goal reactions and, as such, are called into existence by ordinary physical stimulation; and (2) that these anticipatory goal reactions are pure-stimulus acts and, as such, guide and direct the more explicit and instrumental activities of the organism. In short, ideo-motor acts, instead of being *evoked* by ideas, *are* ideas. Thus the position of the classical psychology in this field is completely inverted.

Healthy individuals, uncontaminated by psychological and metaphysical theory, generally have quite simply and naturally held that ideas were dynamic—that somehow they exert a physical control over instrumental activity. The prevailing metaphysics, with its roots far back in the unscientific past, has insisted that ideas are not physical. The combination of the two points of view produced the logical absurdity of hypothesizing a physical interaction where one of the two entities involved was non-physical. Among other difficulties, such an interaction found itself in direct conflict

with the principle of the conservation of energy. The present hypothesis, according to which the idea [13] is physical and at the same time an action and a stimulus, completely dissolves this logical absurdity, opens the door for an objective study of ideas, and offers a legitimate and unambiguous status for them in science.

But granting, for the sake of argument, that a non-physical idea could somehow evoke physical movements, there still remains the fundamental question of how, in detail, such an idea could evoke the particular actions which would be necessary to lead to a particular goal even in the relatively simple situations considered above. Schools of psychology dominated by metaphysical idealism have been peculiarly insistent upon the obligation of physical psychological theories to explain the guidance or control of action by ideas. The fact that the mentalists themselves have not been able to do this seems hardly to have been noticed, either by themselves or anyone else. Possibly in a system already filled with the incredible an additional incredibility attracts little notice. The fact remains that there exists no magic which absolves *any* system which purports to give a thoroughgoing account of human nature from the obligation of showing how purposive ideas [14] are able automatically to guide and direct action to the realization of a goal or reward. The problem is admittedly a difficult one. However, considerations already put forward in this paper furnish some grounds for optimism concerning its final solution on a strictly physical basis. At any rate the challenge is accepted.

[13] It is not to be understood that the anticipatory goal reaction is the only physical basis for what have in the past been called ideas. There certainly are some others and probably a good many others, particularly with human subjects. What was lumped together by the classical psychologists as a single thing is turning out to include a number of fairly distinct things. Thus the old terminology becomes inadequate for modern needs.

[14] In a previous paper purpose was tentatively identified with the drive stimulus (S_D). Upon more mature consideration the writer is inclined to revise this judgment in favor of the goal stimulus (s_G), largely on the ground that the latter stimulus both represents the goal and provides the more intimate mechanism of its attainment, even though the goal stimulus is ultimately dependent for its existence upon the drive.

VII

We may now briefly summarize the results of our discussion:

The drive stimulus accounts very well for the random seeking reactions of a hungry organism, but alone it is not sufficient to produce the integration of complex behavior sequences such as is involved in maze learning. There must always be a reward of some kind. Once the reward has been given, however, the behavior undergoes a marked change most definitely characterized by evidences of actions anticipatory of the goal, which actions tend to appear as accompaniments to the sequence ordinarily leading to the full overt goal reaction.

It is shown how these fractional anticipatory goal reactions could be drawn to the beginning of the behavior sequence and maintained throughout it by the action of the drive stimulus (S_D). The kinaesthetic stimulus resulting from this persistent anticipatory action should furnish a second stimulus (s_G) which would persist very much like S_D. These two persisting stimuli alike should have the capacity of forming multiple excitatory tendencies to the evocation of every reaction within the sequence. They should differ, however, in that the anticipatory goal reaction stimulus would be dependent for its existence upon the integrity of the drive stimulus. A second difference is that a single drive stimulus may generate many distinct goal stimuli.

The general *a priori* probability of the existence of the goal stimulus finds confirmation in the fact that it affords a plausible explanation of a class of experimentally observed facts hitherto inexplicable. It enables us to understand, for example, why withholding the usual reward at the end of an accustomed maze run will cause a disintegration of that particular habit sequence while leaving the organism free to pursue alternative sequences based on the same drive. It offers an explanation of why, during a maze learning process, the substitution of one reward for another presumably of about the same attractiveness should produce a transitory slump in the learning scores. It throws light on why an animal evidently motivated by the anticipation of one kind of

food will leave untouched a different but otherwise acceptable type of food which has been surreptitiously substituted. There is reason to believe that as the experimental literature on the motivating influence of rewards increases the goal stimulus mechanism will find enlarged application.

Moreover, there is strong reason for believing that the fractional anticipatory goal reaction is the actual basis of what has long been known as ideo-motor action. This latter phenomenon emerges from the analysis as a dynamic mechanism, a pure-stimulus act, rather than an end product as was formerly supposed. This means that ideo-motor acts are not caused by ideas. On the contrary, they are themselves ideas. It has long been recognized that one of the prime functions of ideas is to guide and control instrumental acts in cases where the situation to which the acts really function is absent and, as a consequence, is unable to stimulate the organism directly. The capacity of anticipatory goal reactions as stimuli to control and direct other activity renders intelligible on a purely physical basis the dynamic guiding power of ideas. This, in turn, makes still more plausible the hypothesis that anticipatory goal reactions are the physical substance of purposive ideas.

For the sake of definiteness and additional clarity the hypotheses elaborated above may be assembled in brief dogmatic form: Pure-stimulus acts are the physical substance of ideas. Ideas, however, are of many varieties. Among them are goal or guiding ideas. The physical mechanism constituting these particular ideas is the anticipatory goal reaction. This appears to be substantially the same as ideo-motor action. The anticipatory goal reaction seems also to constitute the physical basis of the somewhat ill-defined but important concept of purpose, desire, or wish, rather than the drive stimulus as has sometimes been supposed, notably by Kampf. This interpretation of purpose explains its dynamic nature and at the same time removes the paradox arising under the classical psychology where the future appeared to be operating causally in a backward direction upon the present. This hypothesis also renders intelligible the 'realization of an anticipation' by

an organism. It is found in situations where a fractional anticipatory goal reaction as a stimulus has motivated a behavior sequence which culminates in a full overt enactment of a goal-behavior complex of which it is a physical component.

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THE SEQUENTIAL METHOD FOR THE STUDY OF MATURING BEHAVIOR PATTERNS

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For some years psychologists spread abroad the hypothesis that complex behavior as seen in the adult is built up through a linking together of the simple reflexes which are present in the newborn child. The hypothesis had wide acceptance because it seemed simple and logical, and because it held out the hope that by assiduous observation the student of behavior would sooner or later espy two reflexes in the process of becoming chained together in much the same objective way as the biologist now observes mitosis. Once the chaining process should be observed then an inroad would have been made and it would be but a matter of time until the entire way from the simple acts of babyhood to the complicated behavior of adult life would lie along a well-mapped route.

Recently, however, observers have shaken that hope by their discovery that reflexes are not the simple units upon which all other acts are based, but instead that they themselves are differentiated out of more generalized activities which involve coordination of the body as a whole. This narrowing down of behavior from the whole to the part, which takes place in those animals in which it has been studied in embryonic life, is quite evidently a manifestation of normal growth processes. Thus the chain reflex hypothesis now topples on its pedestal. The proof that with growth reflexes become 'individuated' out of the earlier, organized reactions of the organism as a whole leads one to ask whether some of the complex activities which appear later in the life cycle may not also be the product of an organization of behavior through growth rather than through experience. Hence the building up of behavior patterns through growth, a phenomenon which

may conveniently be spoken of as *maturation*, is today a hypothesis the proof of which is of wide-spread theoretical, as well as practical, importance.

I

Any consideration of the maturation hypothesis, however, brings up the age-old controversy concerning heredity and environment. Ten years ago, under the leadership of Watson, Kuo, and others, a reaction against *instinct* as a useful psychological concept swept over the country. The more radical writers insisted that the distinction between native and acquired traits be completely abolished, and the more conservative adopted the view that heredity and environment are so intertwined that it is impossible to evaluate the influence of either separately. Carmichael (7) in 1925 marshalled the evidence against the theory of 'preformed bonds' and other distinctions between native and acquired characteristics which earlier writers had formulated. He then clearly stated his allegiance to the theory of interaction between heredity and environment: "The fact as it appears to the present writer is that no distinction can be expediently made at any given moment in the behavior of the individual, after the fertilized egg has once begun to develop, between that which is native and that which is acquired." This view would force one who sought to determine hereditary influence apart from that of environment back to the original parent germ cells, the study of which, in the present state of our knowledge, is extremely difficult and indeed impossible in any but the lower forms of life.

We are indebted to Marquis (21) for a discussion wherein he draws a valid distinction between native and acquired traits and clears the road for the genetic psychologist to attack the maturation problem. Without discounting the theory of interaction between heredity and environment, he clarifies it by dividing environment into three aspects, intra-cellular, inter-cellular, and external. The last environmental category, the outside world, is often the only one taken into account by the psychologist and the sociologist since it is the

only one which they can conveniently measure. Furthermore Marquis emphasizes the fact that intra-cellular and inter-cellular environment are remarkably constant. The individual organs and their component cells are protected from the ravages of external environment by regulating mechanisms which keep body temperature constant and internal media at optimal concentration. Only after considerable delay can changes in external environment filter through to organs and cells.

Moreover Marquis points out that in all species the embryo is further sheltered from external environment by being tucked away in a safe spot during its development. The mammalian embryo is doubly protected since environmental influences can operate on it only after having altered the blood stream of the mother. The author further says that the individual as truly 'inherits' his intrauterine environment as he does the plasm of the two germ cells which were his origin; he suggests *beta heredity* as a term to designate this feature and *gamma heredity* as a name for the social milieu into which the child is born.

In view of these facts Marquis defines *maturation* as development carried out by the interaction of the organism with its inner environment. This lucid definition allows us to surmount the stumbling-block, convergence of heredity and environment, and makes the evidence favoring maturation as an explanatory principle of complex behavior less subject to the attacks of environmentalists and interactionists.

During the last few years this evidence has mounted to surprising proportions. Most noteworthy are the researches of Coghill (11) on Amblystoma Punctatum embryos in which he has correlated motor behavior with neural growth, and from which he has derived the principle that integrated movement of the entire body develops first and that individuation of reflexes comes later. He has also shown that motor nerves make contact with the muscles before sensory nerves connect with them centrally. Hence the moment sensory excitation can get through, reaction mechanisms are ready for response. The significance of findings such as these for the maturation hypothesis is indicated above.

Further support has been lent this hypothesis by Carmichael's (8, 9, 10) discovery that Amblystoma raised in chloretone solution swim as soon as the narcosis wears off, and that those raised in a dark, vibration-free room also swim as early and as successfully as those kept under ordinary laboratory conditions. Still adhering to the interaction theory, Carmichael nevertheless grants that typical behavior patterns develop even under great variations of environment. Tilney and Casamajor (29) and Langworthy (19) have contributed much through their studies of myelinogeny and behavior development in kittens and opossums. Quite as important as their neurological findings is their observation that a definite chronological sequence is manifested in the appearance of behavior patterns. Avery (2) has established the same fact in his work on fetal guinea pigs, and Swenson (28) has similar evidence on rat fetuses. These recent successful attempts to correlate developing behavior with developing nervous system were foreshadowed by Lui (20) in 1894 when he compared cerebellar development of the infant, the puppy, the lamb, and the pigeon at the age at which each species walked; and later by Addison (1) who found that "the albino rat is in full possession of its motor powers when the cerebellum has attained its mature arrangement."

Maturation of the pecking response in chicks has been studied by Shepard and Breed (25) and by Bird (3). The latter believes that physiological development, rather than practice, is responsible for the differences in accuracy of pecking which he finds between first and second day chicks; maturation is less effective after the first two days, however. Yerkes and Bloomfield (30) watched the development of mouse-catching behavior in kittens and found that although it was not present during the first four weeks it did appear in a complex form shortly thereafter. These experiments conducted on animals have led their authors to pin considerable faith to the maturation hypothesis.

In spite of the obvious impossibility of submitting infants to the same types of rigid experimentation, considerable data have been amassed which bespeak the importance of matura-

tion in human development. The work most closely akin to Coghill's is that of Minkowski (**22**) and of Bolaffio and Artom (**4**) on spontaneous and elicited movements in accidentally aborted fetuses. In general, activity of the smaller fetuses (under 10 cm.) is diffuse, involving the body as a whole; in the larger and older ones many well differentiated reflexes are brought forth by appropriate stimulation. These results are consistent with Coghill's observations of the developmental order in amblystoma. Peiper (**23**) gives an adequate summary of the activities and reflexes of prematurely delivered fetuses, 1000–2000 grams in weight, of the unborn child during late pregnancy, of the mature newborn, and of the baby during each of the four quarters of the first year. For each age level he notes the movements which are present, those which are beginning to appear, and those which are disappearing. For prematures of this size Peiper describes more than a dozen well-defined reflexes, some of which, notably assymmetric tonic neck reflexes similar to those which Magnus found in decerebrate cats, are on the wane in the child born at term.

Another study of infants which in its author's opinion lends support to Coghill's theory is that of Irwin (**17**). He observed and recorded, both by notations and by stabilimeter tracings, the spontaneous activities of four infants constantly over a 10-day period. These activities were classified into two types, mass activity which designated a general squirming, and segmental activity which involved movements of specific body parts. Although the latter were present in considerable numbers, the former greatly predominated at this early period. Another interesting result was that movements of the anterior part of the body outnumbered those of the posterior section after the first day.

An attack upon the maturation problem from an entirely different angle has been made by Gesell and Thompson (**14**) and by Strayer (**27**). Gesell invented the co-twin control method, wherein one member of a pair of twins, previously shown to be identical both by physical and mental tests, is trained in a given trait and the other is kept as a control.

After six weeks training in block-piling the trained twin showed no superiority to the control; the improvement noted in each may as well be attributed to development as to practice. The control twin was also able to scale the experimental stairs without previous practice, although she was not so proficient as her trained sister. In two weeks of practice at the later age, however, she made up the deficiency which had arisen from the six weeks of training given to her twin at an earlier age. Not even language development is free from the effects of maturation, although in this trait the influence is less marked. Strayer gave Twin T intensive training in language between the ages of 84 and 88 weeks; she then duplicated the training on Twin C between the ages of 89 and 92 weeks. C's greater maturity enabled her to acquire speech at a more rapid rate during her training period than had her sister at the earlier period. T's language development, however, was not arrested during C's training period, and consequently at 92 weeks her total achievement was greater than that of C. This discrepancy between the twins had practically disappeared after three months. Another study from Gesell's clinic is that of Halverson (**16**) on the development of prehension. Definite steps of progress toward prehension were observed, the consistency of which seems to place this in the category of maturational rather than of learning processes.

II

Important and suggestive as they are, these investigations on humans are not nearly so complete and conclusive as are those on animals. Obviously the chief reason lies in the difficulty of finding methods for studying maturation at the human level. Coghill's method can be used only for species in which the embryos are viable in external media; and Minkowski's method can have only limited application because living fetus are difficult to obtain and because they are short-lived after delivery. Under present social conditions it is also impossible to alter the environment of babies so radically as Carmichael has done for his anaesthetized salamanders.

Irwin's technique for recording activity could hardly be applied to the problem of maturation as such, although it offers interesting possibilities for a study of individual differences. Unquestionably Gesell's co-twin control method presents the best opportunity for comparing the effects of training with those of mere development. But its application on a large scale is prohibited by the fact that identical twins are scarce and hard to find, especially ones whose parents are willing to yield them up for long-continued and crucial laboratory experiments. It therefore behooves psychologists who are interested in the study of maturation to seek a new method which can easily and safely be applied to large groups of children.

The sequential method about to be described offers such a possibility. Avery, Tilney and Casamajor, and Swenson have dwelt upon the existence of an orderly sequence of behavior development in the animals which they have observed. Gesell (**15**, p. 307) puts emphasis upon the sequential development of prehension as found by Halverson. Concerning the sequence as a criterion of maturation Gesell writes: "These patterns of behavior (reflexes as described by Minkowski) follow an orderly genetic sequence in their emergence. Genetic sequence is itself an expression of elaborate pattern. And the relative stability of both prenatal and postnatal ontogenesis under normal and even unusual conditions must be regarded as a significant indication of the fundamental role of maturational factors in the determination of behavior." None of the Yale studies, however, have employed the sequential method exclusively nor have they elaborated plans for its use.

If a sequence of development is to be used as evidence for the maturation hypothesis its existence must be rigidly established. It seems to me that this can best be done by a longitudinal study wherein a sizable group of infants are observed over a long period of time with regard to a large number of traits. The wide variability among babies in the age of development of a given trait makes age a confusing element in a sequential study. The steps of the sequence, therefore, should be defined and described in such a way that the se-

quential order of development, rather than age of development, is the thing considered. Greater weight will be given to the maturation hypothesis if there is wide variation in the environmental conditions under which the different infants are developing; for a consistent sequence under diverse environmental stimulation cannot be attributed to environmental influences.

In drawing up specifications for the sequential method one must consider also the number of cases necessary to establish a sequence. It is difficult to say just how many cases are needed for any given study. The more the better, of course. But it would seem that fewer cases would be necessary to establish a reliable sequence than to establish age norms for development. In general the number of cases needed will decrease with increasing disparity of the sampling, in regard to social and environmental conditions, with increasing number of items in the sequence, and with increasing concentration of the items within a narrow age range. For if a sequence holds for individuals at opposite ends of the social, economic, and intellectual scales, or if it holds for different racial or cultural groups, then it must be fundamentally sound. Mathematically the more items a sequence contains, the less likelihood there is of its being established by chance; three items completely permutable may stand in six possible sequences, 5 items in 120, and 15 items in more than 8 billion. Assuming that the items were not interdependent, the possibility that a consistent sequence would occur through chance alone is extremely slight, provided it contained five or more items. Finally, if the items of a sequence are separated from each other by wide intervals of time in the average age of development, the sequence is much less impressive than if the same number of items are concentrated within a short interval. For example, the sequential occurrence of five items spaced at intervals of 3 months, such as eye coordination established at an average age of 1 month, reaching for toys at 4 months, sitting alone at 7 months, creeping at 10 months, and walking alone at 13 months is less significant than a sequence of the same number of items concentrated within

three months, as eye focusing 2 weeks, eye following horizontally 5 weeks, eye following vertically 7 weeks, propping on elbows when prone 9 weeks, and eye following horizontally 10 weeks. The sequence assumes greatest importance when it can be established for a large number of items spaced at frequent intervals. If the sequential method should be adopted for general use, no doubt statistical methods could be worked out for weighting these three factors, environmental disparity, number of items in the sequence, and frequency of their spacing, and definite standards could be set for the number of cases necessary to establish a sequence.

In addition to a criterion for reliability of a sequence there needs also to be a criterion for its validity. At present the best measures we have of extent of maturation are physical and anthropometric. A developmental sequence in order to be valid should, therefore, conform to the established principles of physical growth.

At best the existence of a sequence can never be used as conclusive proof that maturation rather than experience is largely responsible for the course which development takes. A single environmental factor common to all the individuals in the group studied might account for the sequence. It would seem, however, that if experience is the more important factor there would be many cases in which one state or another in the sequence would be skipped. To be sure development would not be so chaotic as it would if chance alone were the guiding principle; but one would hardly expect to find a consistent and orderly sequence as a product manufactured primarily by diversified experience.

The remainder of this paper will be devoted to a brief description of a sequential study and to a cursory statement of some of its results.

III

At the time of its establishment, the Institute of Child Welfare at the University of Minnesota chose as one of its major projects a study of infancy. In 1927 plans were afoot to make the study a longitudinal one in which babies would be examined at frequent intervals in their homes. The work

was turned over to two investigators, a pediatrician and a psychologist. Through cooperative obstetricians contacts were made with prospective mothers, and of the thirty-odd to whom the study was outlined, twenty-five readily consented to have their babies included in the experimental group. The examiners made their first observations in the hospitals, taking records daily during the first week, and every two days during the second. Thereafter they made weekly visits to the home for the first year and biweekly the second. Throughout this period physical examinations and anthropometric measurements were made at intervals of four weeks. The remainder of the visits were given over to psychological examinations the contents of which were changed at appropriate and frequent intervals in order that the test materials and procedure should always be suited to the babies' level of development and sufficiently novel to command their interest.

A few of the advantages of these experimental conditions are that the same babies were examined over a long period; that physical and psychological traits were observed in the same group of children; that both observers were present at all examinations, the work being divided so that the pediatrician did all the handling of the baby and the psychologist took all the notes, supplementing her own observations with those dictated by her co-worker; and finally that the baby was at ease in his familiar home surroundings, and the examiners soon became as familiar to him as neighbors and relatives. Some disadvantages are likewise readily apparent. The frequency of the visits and the distances which had to be traversed between homes limited the number of cases which could be handled to twenty-five. Home visits precluded the use of all but the simplest and most portable pieces of apparatus; and rigid control of such factors as sounds, lights, and presence of an audience, such as is possible in a laboratory, was absolutely impossible in the homes.

Bearing these facts in mind, let us see to what extent this study meets the foregoing requirements as to size and disparity of the sampling for a sequential study. The number of cases, 25, is certainly too few for a normative study; but since

it is logical that fewer cases are needed to establish a sequence it is probable that the number is adequate. Concerning the requirement of disparity of the sampling it must be admitted that these 25 babies were a select group. All were drawn from the upper three occupational classes, according to the classification used in Minnesota studies. They were selected also by the factor of the mothers' interest and willingness to cooperate. All were of white parentage, and only one parent was foreign-born. Among the nationalities and racial stocks represented in the forebears were English, Scandinavian, German, French, Irish, Scotch, Russian, and Jewish. The number of siblings varied from four to none, and the age of siblings from 14 years to 13 months older than the baby studied. In the matter of infant care feeding was probably the most uniform feature for the group. Marked variations occurred in daily schedule, clothing, toys, playmates, and training methods. Although the group represents a less disparate sampling than might be desired, yet it is no more circumscribed than are the majority of groups on which infant experiments are made.

Perhaps it will be well at this point to allay some doubts and criticisms which may have arisen in the reader's mind: the first that one who embarks upon a study in the hopes of discovering a sequence will, consciously or unconsciously, arrange experimental conditions so that he will find it. To this charge the writer must confess, even at the risk of making herself seem slipshod in her scientific attack, that she began the study with no preconceived idea of its purpose, and with no particular theory importuning her for proof. Her plan was simply to watch the babies as carefully and scientifically as possible and to see what happened. It was not until the middle of the second year, when the bulk of the data were gathered and were undergoing treatment, that she clearly formulated the two major objects of the study: namely, to trace the course of infant development, and to study the consistent manifestation of personality traits in individual babies.

A second criticism might be that the developmental se-

quence was merely a by-product of the types of tests used; and that a different series of tests might have yielded a very different sequence. This may be answered by considering a few facts about the tests. In the first place they were simple, and involved a type of handling similar to what the baby received during his daily care. Furthermore the tests were not scored as passed or failed, but instead the reactions of the baby were jotted down in consistent descriptive terms, and the reactions afterward were analyzed term by term. Finally, there were two other important sources of data,—incidental items kept by the observer, which included all reactions made by the baby from the moment the examiners arrived till they left besides those in response to a test situation; and daily and weekly records kept by the mothers on uniform blanks supplied them by the examiners. These incidental items and mothers' records represented spontaneous rather than elicited behavior on the babies' part.

Again, the criticism might be raised that the developmental items included were such that it was logically and mechanically impossible for them to appear other than in sequence. For example, a baby cannot retain an object without having grasped it, cannot grasp it without having reached for it; the sequence reach, touch, grasp, and retain is, then, the only possible one. A valid criticism certainly, but one which does not explain why these four items should be separated from each other by about a week's difference in age of development. Nor does it explain why certain apparently unrelated activities which involve coordinations of entirely different muscle groups should fall in sequence; why watching a person, for example, always preceded propping up on the chest in the prone position, or why smiling always preceded reaching and grasping a toy.

Also one might inquire whether, without being intentionally aware of it, the examiner had not at one age been sharply on the look-out for one phase of development, and thus had observed it in all babies; and a little later had her trained eyes on some other phase, and saw it to the exclusion of other items. Such sets of attention on the observer's part would yield a

sequence which might have no basis in fact. This could hardly have happened, for two reasons; first that the babies were born over a five-months period, and hence they did not pass through the same ages at the same time; and second that the records show wide variations in the ages at which each item was first recorded.

Finally, one might ask whether in analysis of the data items which were inconsistent with the sequence had not been thrown out. Almost 100 developmental items were obtained of which a small number were discarded. Only those items were discarded from the sequence which failed to appear in one-fourth or more of the babies. Unless an item was established at some time or other in 75% of the cases its developmental significance was considered dubious. Such items were then relegated to the category of play responses. Later the play reactions were studied to determine whether they were consistent with the developmental status of the baby at the age of their appearance.

To be more specific as to the method of determining the sequence, the age of first appearance of each developmental item was obtained for each baby; medians were then worked out for each item. Next the sequence was made up by arranging the items in chronological order according to the median age of development. Fidelity to the sequence was then determined by comparing it with the order of development of items for each baby. Pairs of items between the members of which reversal was logically impossible were not treated in this way.

In this manner sequences were first worked out for the development of walking, then for that of creeping, assuming the upright posture, reaching and manipulating, and for eye coordination. It was then evident that these were but a part of a much broader sequence; consequently the items from all were thrown together to form the *motor sequence*. At the present time the items involved in speech development, social development, and that elusive trait which we might call 'seeing the point' are being put in order; and it appears likely that they will dove-tail into one another and into the

motor sequence in such a way that in the end we may have a much more general *developmental sequence of babyhood.*

IV

It is beyond the scope of this brief paper to present the *motor sequence* in detail.[1] Simple motor tests, each one minute long, made up a part of each psychological examination. Eye coordination and reaching were tested for by dangling a shiny metal tape before the baby's eyes and by offering him other instruments and toys. For the creeping test, which was consistently used from birth to one year, the baby was put flat on the floor on his stomach and a small hand-bell was rung and placed just beyond his reach. For testing 'sitting with support' the examiner estimated the amount of support necessary to keep the baby sitting upright on her lap by noting where she placed her hands to hold him; there were five degrees of support,—at armpits, mid-ribs, lower ribs, hips, and no support. Sitting alone was considered as established when the baby sat for one minute on the floor without being propped. Progress toward standing and walking were tested by holding the baby erect for one minute with his feet touching the floor. From the reactions to these tests, supplemented by incidental items and items from the mothers' records, the motor sequence was drawn up.

In its barest essentials the sequence began with eye coordination and head control; in succession upper trunk, arms, hands, lower trunk, legs, and feet came under control. Eye coordination went through five sub-stages: fleeting pursuit movements, fixation on person or object, and eye following a moving object first in the horizontal, then in the vertical and finally in the circular directions. Simultaneously with these last two stages of eye movements came the social smile, and chest lifting in the prone position. Reaching, touching and grasping objects came next in order; control of the upper trunk was further manifested by the babies' ability to sit on

[1] For a more complete account of the sequences see the author's work, The first two years: A study of twenty-five babies, Vol. I., Locomotor development, The Institute of Child Welfare Monograph Series no. 1, University of Minnesota Press. 1931.

the doctor's lap with support at lower ribs only. Retention of an object with thumb opposition and carrying object to mouth indicated that arm and hand had come under control. With the appearance of rolling and sitting alone, motor control had extended down to the sacral region; and with the use of the index finger for pointing, which occurred some weeks later, it had advanced to the finger tips. When sitting alone was an accomplished fact progress toward creeping began in earnest; rolling gave way to 'swimming,' pivoting, or crawling on the belly, and they in turn to scooting backward, and then to creeping. Attempts to assume the upright posture began with the babies' standing firmly when supported at the armpits; this act was interpolated into the sequence just before the babies sat alone. Standing with support of a chair came just before creeping, walking when led was simultaneous with creeping, and pulling to the standing posture from a seat on the floor in front of a chair was manifested by the babies just after creeping. A long gap intervened between this act and that of standing alone, an act that was shortly followed by walking alone.

The sequence, as thus outlined, was determined by the median age of development. It becomes more impressive when one considers the slight number of departures or reversals from the established order. In no case was there a reversal between fleeting pursuit movements of the eyes and fixation on person or object; in only one case a reversal between fixation and horizontal following; and none between horizontal and vertical or circular following of the moving object. In every case lifting the chin preceded lifting the chest in the prone posture; in 15 out of 23 cases chin lifting preceded horizontal eye following, and in 20 out of 23 cases horizontal following preceded chest lifting. Chest lifting and vertical eye following preceded touching an object in 20 and 22 cases respectively out of 23; touching an object preceded sitting with support at lower ribs in 20 out of 22 cases. It is needless to amplify examples; suffice it to say that 21 pairs of items, in 17 of which the two items were separated by four weeks or less in median age of development, and in the other

4 pairs by not more than six weeks, were analyzed for number of reversals; out of a possible 460 only 56 reversals occurred, which amounts to little more than 12 percent. Since the motor sequence is made up of no less than 45 individual items, excluding complex manipulatory items, a total of 990 comparisons between pairs of items might be made. But reversals for items separated by larger age intervals are so scanty that it would be absurd to make all the possible comparisons. Considering the shortness of the time interval between those items which were compared, an interval which for the most part was shorter than the semi-interquartile range for the individual items, the wonder is not that reversals are so many but that they are so few. And it is altogether likely that some of those which did occur are attributable to errors on the part of the observer rather than to reversals in the developmental order.

Supporting evidence for the motor sequence lies in the fact that spontaneous motor play, much of which would seem to have little value as practice for developing motor functions, also develops by stages which fit neatly into the sequence. With the development of reaching, scratching at toys, tabletops, bed-sheets, and the examiner's arm consistently appeared in all the babies. In the interval between the establishment of successful reaching for dangling objects and sitting alone the babies diverted themselves by playing with their toes. Fenton's (12) observation on development of toe-play is of interest here. She writes that she had repeatedly tried to acquaint her son with his toes, but her efforts had been of no avail until at 28 weeks the baby himself succeeded in grasping his toes and getting them to his mouth; thereafter these amusing digits figured 'among his most fascinating playthings.' Toe-play, as observed in the present study, waned with the onset of creeping.

Interesting changes in the babies' reactions to furniture came about after creeping began. Up to this period chairs and tables were their all-but-human helpers. They served the young adventurers as something to pull up by, to lean upon, and to clutch at on the verge of a tumble; and in return

these inanimate objects were patted, rubbed, and smoothed with considerable deference and respect. But at the advent of creeping and walking with help, the babies relegated furniture to the same subservient rôle which it plays with adults. Chairs were things to push and pull about, to climb and bounce upon and to be dragged in front of chests and mantles for use as ladders in scaling to forbidden heights. Drawers, doors, and cupboards were pulled open and their contents explored; and electrical switches, and plugs, and locks and latches were investigated. In fact furniture was exploited for its own sake and used as a tool for further exploiting the environment.

Granting for the sake of argument that the motor sequence is merely an artifact produced by the nature of the test situations, there would still remain the sequence of motor play for the critic to cope with. For motor play, of which only a few examples have been given, was spontaneous; the notes on it were gathered from incidental items and mothers' reports rather than from reactions to the test situations. Nevertheless characteristic play responses were observed to fall in sequence and to accompany their appropriate stages of motor development.

Further evidence that the motor sequence is a fact comes from the biographical records of babies by Preyer, (**24**) Shinn, (**26**) and Fenton. Allowing for slight differences in descriptive terms and in criteria of accomplishment the stages of progressing motor skill described by these authors follow the same order as that obtained in the present study. When one considers that the biographers for the most part did not rely upon motor tests but merely recorded each new phase as it developed, the argument for a sequence becomes even more convincing. Furthermore the sequence accords well with the order of normative items worked out by Gesell (**13**) and with the experimental findings of Burnside (**6**) in her study of locomotion and of Jones (**18**) in her observation of early behavior patterns. Finally Charlotte Bühler's (**5**) recent analysis of 24-hour observations made at monthly intervals during the first year reveals an identical sequence. In these observa-

tions Bühler used no tests, but merely recorded the babies' spontaneous behavior.

The fact that the motor sequence holds not only for one baby but for 25; not only for Minneapolis babies of 1928, but also for German babies of the late 1870's and of 1925, as well as for California babies of the '90's and the '20's; for babies swaddled in long dresses, for babies given the freedom of rompers, and for babies stripped to sun-suits,—the fact that the motor sequence transcends these special conditions makes it impossible of explanation in terms of chance, and casts strong doubt on the theory that it is a product of experience or learning. Motor development sweeps on in an orderly course, apparently little influenced by the exigencies of time, place, and current fashions in child dress and child training.

V

But that *maturation* accounts for the on-sweep of motor control cannot be affirmed with confidence until the validity of the sequence as a manifestation of development is tested for conformity with known principles of growth. There is a well-established anatomical law sometimes called the *law of developmental direction*,—a law which states that the growth wave sweeps over the body traveling from head to foot. In the early fetal period the head comprises about one-half of the total body length; by birth other parts of the body have grown more rapidly so that the head is about one-fourth of the total length, and at adulthood it has further diminished in proportion to trunk and limbs until it makes up only one-eighth of a man's stature. At two years the eyes have reached approximately their adult size, and by six years skull and brain are little short of full grown. The adolescent boy and girl quite literally become 'all arms and legs' since the extremities outstrip other parts in growth at this time.

In the unfolding of the motor sequence the operation of the same law is apparent. Assumption of motor control begins headward and travels toward the feet. Beginning with control of eye muscles, and progressing through stages in which head and neck muscles are mastered, arm and upper

trunk come under control, hand and lower trunk become directed, and lower limbs become skilled for creeping, the baby at last achieves mastery of his entire body for upright posture and for walking. The harmony of the sequence with this important growth principle is a strong argument in favor of the maturation hypothesis as an interpretation of motor development.

The reader has probably noted the conspicuous omission from the motor sequence of reflexes and early activity which may be designated as spontaneous or somatically stimulated behavior, or, to use Irwin's terms, mass activity and segmental activity. These reactions were by no means overlooked. Foot-sole and tendon reflexes were consistently tested for at the regular physical examinations; their presence and their variability will be discussed in connection with that phase of the study. Body posture in the crib was noted at the beginning of each examination, and squirming, kicking, waving, finger and toe fanning were observed and recorded. Since all these activities were present at birth they cannot be fitted into this motor sequence which extends back only to birth. The works of Coghill, Minkowski, and others mentioned above can much better locate these activities into the developmental scheme. Irwin has studied spontaneous movements in the neo-natal stage much more adequately and comprehensively than I can hope to do from this work. It is hoped, however, that the data will repay the quantitative treatment, to which they are now being subjected, by throwing light on the tremendous individual differences in rate of motor development that obtain in spite of the consistency of the sequence.

Furthermore it seems to me that a distinction not only should but must be drawn among reflexes, random activity, and controlled activity if the entire course of motor development is to make sense. To be sure it is difficult to definitely distinguish the different types. Reflexes are most easily differentiated from the other two by their pattern-like character, and by the specificity of the stimuli which arouse them. The other two types are harder to separate. Those move-

ments which are present at birth cannot be called 'uncoordinated,' for Coghill and others have shown that they are coordinated; nor spontaneous for they also appear in response to stimulation; nor random, for in many instances they are aimed at the removal of an irritating stimulus; nor involuntary, for the same reason, and for the additional reason that the term voluntary is in ill-repute because of its philosophical implications. The least objectionable terms which occur to me are 'coordinated but undirected or unsuccessfully directed' as against 'successfully directed' reactions. Using these somewhat awkward names one might say that motor behavior has its genesis long before birth in 'coordinated but unsuccessfully directed' reactions; that from these, according to the observations of Coghill, are differentiated the reflexes; and that last of all 'successfully directed' reactions are developed. No one type of reaction every wholly supplants the others, but shortly after birth reactions of the successfully directed type begin to predominate in infant behavior.

VI

This paper by no means attempts to say the last word on the topic of maturation's role in infant development; nor does it claim to offer conclusive proof that maturation plays a greater part than experience in motor development. Such conclusions would be far too sweeping. It aims, rather, to suggest the sequential method as one that may have wide applications and that promises to bear fruit in studies of maturation at the human level. It points out some of the requirements for the use of this method, and stresses in particular the need for checking the validity of the sequence as a function of maturation against the established facts of anatomical and physical development; and it shows that when such a sequential study was made and the evidence weighed in the balance against a fundamental growth law it was not found wanting.

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THE OBJECTIVE CHARACTER OF LEGAL 'INTENT'[1]

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Introduction.—It seems that one of the misunderstandings serving as obstacles to a more widespread acceptance of the general mode of description of human psychology known as the 'objective' is the continued use of terms ordinarily assumed to be of subjective significance only, but which upon scrutiny turn out not to be so in their actual and complete implications. Such terms are met with not only in popular speech but also in professional parlance. It is the plan of this paper to examine, for a sample, the concept of 'intent' as employed in courts of law.

I. *In Many Cases of Litigation, 'Intent' Is Wholly Immaterial.*—Where asserted violation of *contract* is the point at issue, what the court is bent upon determining is not 'What were the states of consciousness in the contracting parties' minds?,' but 'What is the plain and apparent meaning and purport of the contract as it stands? What was each party to do, as set forth therein?' In cases involving written contracts, any intention of the parties that is not expressed or logically implied in the contract is treated as irrelevant; and even in cases of oral contracts the objective of court procedure is to determine the precise form or nature of the agreement. 'States of mind' are not essential to this inquiry; whereas words and other symbolic reactions as well as a party's demeanor subsequent to the agreement are material proof of 'intent.'[2]

[1] The writer is indebted to Professor R. H. Wettach of the School of Law, University of North Carolina, for helpful suggestions.

[2] It is pertinent to observe that the English word 'intent' has two different meanings, the one being equivalent to 'desire' or 'purpose' (cf. 'intention'), the other synonymous with 'purport' or 'meaning' (cf. 'intension'); and that the second is the truer interpretation of the word as used here.

In many forms of misdemeanors also, the psychological intent of the accused is irrelevant. Violations of highway and parking rules, as well as of other forms of police regulations and local statutes, incur certain penalties administered more or less automatically, regardless of any plea of 'not intentionally' doing the act. If one drives forward against a red stop signal at a street intersection he cannot legally escape punishment by showing that he did not intend to violate the rule. The rationale of the matter would seem to be all of a piece with that of the fundamental assumption in all jurisprudence, *ignorantia legis neminem excusat;* namely, that the law is primarily and ultimately interested in acts and in their effects upon others—not in moral distinctions of states of mind.

Similarly for many of those *torts* committed against other private individuals. "If one raises a club over the head of another, saying that if the latter speaks he will strike, and the person menaced thereupon keeps quiet in order to avoid harm, the assailant is guilty of an assault. He cannot escape liability in such case by saying that he did not intend to strike if the condition of his command had been violated." [3] Again, it is the practical effect and consequences, not the quality of morals, that is of direct concern to the court.

Particularly clearly are these general considerations brought out in recent discussions as to the nature of *negligence* as an element in tortious conduct. "The law, in formulating its theory of liability for negligence in civil cases, has not regarded the mental attitude of the wrong-doer, but has contented itself with fixing an external standard of conduct as the criterion of blameworthiness." [4] "Negligence neither is nor involves . . . any . . . mental characteristic, quality, state, or process. Negligence is unreasonably dangerous conduct—*i.e.*, conduct abnormally likely to cause harm. Freedom from negligence . . . requires only that one's conduct be reasonably safe—as little likely to cause harm as the conduct of a normal person would be." [5] Or again, "Danger, reasonably to be

[3] T. A. Street, Foundations of legal liability, Vol. I., 11–12.

[4] C. Morse, The psychology of negligence, *Canada Law J.*, 1905, 41, 245.

[5] H. W. Edgerton, Negligence, inadvertence, and indifference; the relation of mental states to negligence, *Harvard Law Rev.*, 1926, 39, 852.

foreseen at the time of acting, is the established test of negligence." [6]

The present writer suggests that assumptions as to the precise state of mind of the negligent man tend to be gratuitous, since it is readily conceivable that the same act (ruled to have been negligent by a court) may have occurred in consequence of widely varying attitudes and acts by the accused. The street car motorman may have 'negligently' run into an automobile standing on the car tracks (*a*) when he was fatigued and drowsy, (*b*) while intoxicated, (*c*) while he was in the act of sneezing, (*d*) while building air castles, (*e*) while he was glancing backward into his car to learn the cause of a commotion there, (*f*) while he was springing to the assistance of an old lady tripping at the car door, (*g*) while he was so fearfully alive to the possibility of hitting the auto that the fear functioned as a temporary obsession,—etc., etc. It is submitted, now, that psychologically there is little in common—of a positive sort, at least—in these several cases; and the motorman's 'negligence' is to be determined not by an inquiry as to what was 'in his mind,' but by noting the dangerousness of the act—*i.e.*, its dangerousness in a situation where the great majority of motormen would have behaved otherwise. In short, "Negligence is conduct which involves an unreasonably great risk of causing damage . . . negligence is conduct, not a state of mind." [7]

II. *A Formal Legal Requirement of Intent Is Sometimes Supplied by a Fiction.*—Even when the intent of the accused is formally necessary to a proper classification of his act, recourse is frequently had to the device of supplying his intent, assuming it. This doctrine of 'constructive intent' is applied in such cases as the following. (*a*) Defendant gave to his wife candy he knew to be poisoned; but she, in his presence, handed it to her daughter, who upon eating it died; and the defendant was judged guilty of murder. (*b*) A aims a pistol at B, B snatches up a small child standing near and holds her before him, the bullet from A's pistol kills the child; and both

[6] E. R. Thayer, Public wrong and private action, *Harvard Law Rev.*, 1924, 37, 325.
[7] H. T. Terry, Negligence, *Harvard Law Rev.*, 1915, 29, 40.

A and B are held guilty of murder, both of them being presumed to have intended her death. Thus, a man's responsibility varies with the proximity of his act to the anti-social effects.

Here, although proof that the accused intended the actual consequences of his act is not actually supplied, in order to bring the act under a certain classification of crime this proof is merely assumed. And fundamental to it all is the consideration that what interests the law is the dangerousness of the act coupled with the high probability of its leading to the consequences described.

More general in application is that legal assumption that "a man must be conclusively presumed to intend the natural and proximate consequences of his acts." And this can mean only that the primary interest of the state is in a man's overt behavior as it becomes anti-social in its effects, his responsibility varying with the proximity of his act to those anti-social or dangerous effects.

III. *Even Where True and Actual Intent is Legally Required It Can Be Determined Only Objectively.*—Let us suppose that the accused in the witness box confesses to having done an act but denies any intent to do it, while the prosecution seeks to establish the intent. Now, if what the prosecution is trying to prove is the existence of a certain 'state of mind' in the accused at a certain time, strictly considered, the task is impossible. For only the accused can introspect upon his 'state of mind'—and he denies the 'presence' there of 'intent'! Not even the marshalling of eye- and ear-witnesses can bring the court one step nearer to the accused's 'conscious state,' for their testimony on this point can be only their inferences from his observed behavior at the time, including his verbal behavior. Even his earlier verbal responses avowing intent are behavioristic data, and may fail of truly representing his 'state of mind' by reason of poor introspection, of poor memory, or even of false confession. "There can be no independent intuitive introspection of accused's mind by Judge and Jury." [8]

[8] G. H. T. Malan, Behavioristic basis of the science of law, *American Bar Assoc. J.*, 1923, 9, 44.

In contradiction the argument may be made that although proof offered in court be itself limited to observations of accused's behavior, nevertheless does not all this procedure imply a mental intent back of it all? But a significant point is neglected in such an argument; to wit: "The subjective intention is itself an inference from a prior inference as to the 'objective' intention. . . . Unless the accused's behavior prior to, at, and subsequent to the act and in the witness-box has that unique and irreducible objective relationship to the act which we may term 'aiming at it,' there is no ground for conviction. 'Aim,' 'end,' . . . or 'purpose' . . . is an objective relation unifying acts in a unique way" [9]—a relation as objective, I may add, as that unifying the particular flexor and extensor thrusts in the food-taking of a hungry rat.

What the court is interested in is whether the behavior of the accused was of such and such a character. The positing of intent as a 'state of mind' and attempts to determine its presence or absence spring after all from the necessity of distinguishing acts superficially similar (A strikes B, L strikes M) but fundamentally different (A strikes B in course of a quarrel, L strikes M in hearty greeting). "Criminal intent, so important in the language of law, is imputed to persons who have conducted themselves after certain fashions. Where criminal intent has any psychological meaning, that meaning is derived from theorizing about the significance of acts." [10]

Wigmore states in unmistakable terms an aspect of the objective meaning of intent hinted at in preceding pages. "The argument here is purely from the point of view of the doctrine of chances . . . which eliminates the element of innocent intent by multiplying instances of the same result until it is perceived that this element cannot explain them all. . . . Thus, if A while hunting with B hears the bullet from B's gun whistling past his head, he is willing to accept B's bad aim or B's accidental tripping as a conceivable explanation; but if shortly afterwards the same thing happens again, and if on the third occasion A receives B's bullet in his body,

[9] Malan, *op. cit.*, p. 44.

[10] R. M. Hutchins, The law and the psychologists, *Yale Rev.*, 1927, 16, 685.

the immediate inference (*i.e.*, as a probability, perhaps not a certainty) is that B shot at A deliberately. . . . It is the improbability of a like result being repeated by mere chance that carries probative weight." [11] The same general point appears in a different way in instances of a single act that does or does not have a probability of producing dangerous results, as when a brickmason working high on a building drops a brick that ricochets from a lower scaffold to the head of a passerby fifty feet away. Here analysis of the one complete incident reveals the very high improbability of the mason's act leading to the serious consequence.

"The best way to find out a man's intention is to look at what he actually did and to consider what must have appeared to him at the time as the natural consequence of his conduct." [12]

IV. *Is the Conception of Subjective 'Intent' as 'State of Mind' Essential and Necessary to Legal Procedure?*—Such a query, though implying a step beyond most of the preceding discussion, seems nevertheless clearly foreshadowed therein. As has been outlined, in many legal cases intent is not involved at all. In others it is introduced fictitiously, while in those remaining (where the term is employed with psychological—but not necessarily mentalistic!—reference) the whole actual procedure of the court is to determine the true character of the overt act. It therefore is legitimate to question whether the existence or non-existence of an individual's 'intentional' state of mind is—in the lawyer's phrase— 'irrelevant, incompetent, and immaterial.'

If a positive answer be provisionally returned to the question stated, certain considerations would appear consonant with that position.

First, it is to be suggested that instead of employing the term 'intent' as if connoting some *thing* in the possession or out of the possession of a given person, it would be at least psychologically more accurate to speak of 'intentional' and 'unintentional conduct.' Used in place of a substantive term,

[11] J. H. Wigmore, Principles of judicial proof, 133–134.

[12] T. A. Street, Foundations of legal liability, Vol. I, p. 5.

a qualifying and adjective term would probably serve to keep clearer the object and end of the evidential process in respect to the matter at issue—namely to determine whether the act as performed was or was not of a certain type, and therefore to receive or not to receive a certain kind of treatment from the court.

Secondly, employment of the term 'intentional' in an exclusively objective manner presupposes reference to certain marks or criteria of intentionality. Can such criteria be made explicit? Four have been strongly indicated in the preceding pages. Conduct (1) that leads to anti-social and dangerous consequences, (2) that leads to them immediately or proximately, and, especially, (3) that has a high probability of leading to such consequences, (4) including such as would be anticipated by the normal or prudent or reasonable or average man (which is the test of unintentional negligence and also is an ingredient in all intentional conduct)—such conduct is of the type that tends to be established as 'intentional.'

The first mark demands no special exposition. It is inherent in the distinction between taking another person's property as a joke and as an act of stealing. It is intricately involved in the question whether aiming an unloaded gun at another person is a tort—on which court rulings have been inconsistent—for although on the one hand an unloaded gun cannot do bodily harm from shooting it can do harm by disturbing the otherwise normal behavior of the other person.

The second mark of intentionality is intimately involved with the first. An automobile runs into and injures a pedestrian when the latter had the right of way. If the pedestrian dies within a day or two at least in part from the injuries received, the dangerousness of the auto driver's conduct is manifestly greater than if the pedestrian dies a year later from a complication of disorders including the injuries aforementioned.

The third mark is of especial importance. It is implied in such phrases as, 'natural and probable consequences of his act'; and it clearly sets off the class of 'accidental' acts. One false entry in the cashier's books may be construed as acciden-

tally made, but several false entries become increasingly less possible of such construction. The dropping of a loaded pistol can produce serious results in far fewer cases than can the pulling of a trigger in one aimed at a person.

And, finally, the fourth mark is implicit in all distinctions between 'intentional' and 'unintentional' from a legal point of view. 'Intent embraces knowledge of results.' The legal standard is always based upon the standard of the community, and is an expression of the general attitude toward conduct as normal and as abnormal. Since the average or normal man may be expected to anticipate the usual consequences of his actions, action apparently in disregard of such consequences is action of special concern to a court. And when a judge or jury is called upon to decide whether an act was intentional they attempt to reconstruct the whole situation with a view to determining how a reasonable or normal person would have behaved. "In Evidence . . . the question is, how do people act under certain circumstances? We must go sooner or later to the psychologists to find out. Not to do so is to remain . . . content with hypotheses based on intuition." [13]

A Non-legal Consideration.—It would seem *apropos* to append a note on the everyday usage of the concept of 'intent.'

The soul of Mediaeval theology, that was its own microcosm temporarily resident among unimportant mundane things, became in the introverted British ethical systems of the Eighteenth and Nineteenth Centuries sublimated into the Good Will. And in professional ethical discourses this concept still figures. But what of its status in popular usage? It may be summarily and abruptly put in the common-enough phrase, 'The road to hell is paved with good intentions.' One is not more clearly damned with faint praise than when the disparaging word is said of him that 'his intentions were good,' or when his eulogy reads, 'He was sincere.'

Confirmatory evidence of the unimportance attached to an individual's conscious intentions appears in the realm of sports. Young children may exclaim—each in his own case—

[13] R. M. Hutchins, The law and the psychologists, *Yale Rev.*, 1927, 16, p. 689.

that 'slips don't count'; but in more mature games, whether outdoor athletics or indoor bridge, one does with complete propriety make capital of his opponent's 'unintended' mistakes. Again, a foul in football or boxing is called by the official upon seeing the act done; and no amount of disavowal of intent on the part of the player concerned nor of faith in his protestation on the part of the official is allowed to mitigate the penalty. The act is judged by its consequences upon other players, the spectators, the game in general.

The question of whether 'he meant to do it' does enter crucially into such situations as humorous ones. When is a joke a 'practical joke,' with the implication of being reprehensible? It is precisely when the cutting of the hammock cord or the surreptitious removal of one's chair are likely to entail serious and immediate consequences.

It is suggested, then, that in the psychology of everyday life (the actual psychology as lived, not the cant of popular systems), 'intent' as the conscious mental state of an individual is decidedly less the object of consideration than is his conduct. And when the intentionality of his behavior does come in for critical evaluation, it is on account of certain consequences that are dangerous, direct in consequent, probable, and predictable.

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THE ADRENAL CORTEX AND EMOTION

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The study of basic factors in strong emotional states has been confined almost exclusively to Cannon and his associates (1). They have dealt with visceral movements and with the effect of adrenin, showing that these are essential factors in emotion. Since more than a decade has passed since Cannon announced his findings, and since there has been no subsequent presentation of any mechanism by which another factor is shown to act, a tendency has grown up to regard emotional reactions as being concerned only with the discharge of adrenin and with visceral movements.

Two other factors have been found by more recent investigators. In 1920 Hammett made blood analyses of certain patients and attendants at the Pennsylvania Hospital, Department for Mental and Nervous Diseases, Philadelphia, and found that there was a definite correlation between emotional instability and the variability of the non-protein nitrogen of the blood (2); and in 1922 Uno showed that there was an increase in the weight of the hypophysis of the albino rat, following a period of excitement (5). Neither of these investigators determined the cause of the relationship found in his work; Hammett even brought forth his results as a proof that mental states can affect bodily constitution, apparently overlooking the fact that the variability of the nitrogenous materials might affect the reactions of the individual (4).

While the relationship of the pituitary gland to emotion seems to remain somewhat of a mystery, recent investigations offer a basis for the formulation of a hypothetical mechanism correlating emotional instability and variability of non-protein nitrogen in the blood.

Wyman and Walker have shown that, following suprarenalectomy in rats, there is a marked rise in the non-protein

nitrogen and urea of the blood. The amount of increase tends to parallel the severity of the symptoms of cortical insufficiency, and the increase does not appear when successful cortical transplants are made. Hence they ascribe this rise in non-protein nitrogen to a lack of the hormone from the adrenal cortex (**7**).

It seems, therefore, that the adrenal cortex has the function of regulation of non-protein nitrogen in the blood, and that it is capable of producing the variations which were noted by Hammett. If we assume that it is the adrenal cortical hormone which produces these variations, we are at once confronted by the question: Is the hormone excitatory or inhibitory? Obviously, either type of hormone could produce variations in the constitution of the blood.

Wyman and Walker also found that a lack of the cortical hormone produced a drop in the blood sugar values, and that in this case also the change was proportional to the severity of the symptoms of cortical insufficiency (**6**).

This would seem to solve the problem, for hyperglycemia has been shown to be definitely associated with periods of emotion, and we therefore assume that the cortical hormone acts in conjunction with adrenin to increase the supply of blood sugar. On this basis we assume the cortical hormone to be excitatory in its action.

If a lack of the cortical hormone was shown to produce a rise in the non-protein nitrogen in the blood, and a fall in blood sugar values; then in periods of excitement the reverse should take place; *i.e.*, there would be a fall in non-protein nitrogen and a rise in blood sugar. Since Hammett shows that the more excitable persons have the greatest variability in non-protein nitrogen, we should find that there would be a low mean value for such nitrogenous materials in excitable persons, coupled with a high variability; and that in calmer individuals there would be found a lower variability and a higher mean value.

This, of course, is contrary to the normal situation in which variability is roughly proportional to the size of the mean value, but it is what we actually find in this case. Unfor-

tunately, in discussing the relationship between emotional instability and nitrogenous materials in the blood, Hammett neglects to include the mean values for his cases. But in an earlier account of the same experiment he gives complete blood analyses of nine of the cases (**3**). If we examine these tables we do not find a one to one correspondence (because of 'insufficient sampling?), but we do find there is a rough inverse relationship between mean values and variability. Arranging the cases in order from lowest to highest values for non-protein nitrogen, we find:

First three cases; average N 35.2, average deviation 7.03
Last three cases; average N 44.8, average deviation 6.3

And if we take the highest and lowest cases the differences are even more striking:

Lowest case: average N 34.1, average deviation 9.6
Highest case: average N 54.6, average deviation 1.8
(Figures are Mgms.)

Thus we find that the results are in harmony with the hypothesis that the adrenal cortical hormone is responsible for the variations in the non-protein nitrogen of the blood, and that the cortical hormone acts as an excitatory agent in emotional states.

In cases involving muscular reaction to stimuli which arouse emotions, this assumed function of the adrenal cortex would be advantageous. The reduction in the quantity of non-protein nitrogen would enable the blood to remove more rapidly the waste products of the muscle tissue. The medullary portion, furnishing adrenin which aids in the release of sugar, and the cortical portion, which clears the blood of impurities, would be called into action simultaneously, as the nerve supply is in general the same.

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THE RELATION OF BORDER-CONTRAST TO THE DISTINCTNESS OF VISION

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I. Introduction

When the normal man looks at an object, the image he perceives is distinct in spite of the fact that the optical system of the eye is subject to the defects of chromatic and spherical aberration,[1] and that the retinal image of an external object must always have a more or less ill-defined border on account of the inevitable irradiation of light. Why in spite of this irradiation of light do our visual images appear sharply defined? Myers suggests that border-contrast compensates for the ill-defined border of the retinal image and thus "subserves the important biological function of sharply outlining seen objects." [2]

In order to establish the validity of Myers' hypothesis it is necessary to demonstrate that a graded stimulus can yield a visual image with a distinct border; for border-contrast as ordinarily conceived is merely contrast at the border and does

[1] When light composed of different wave lengths passes through an ordinary double convex lens, the shorter waves (violet) refract more and come to a focus sooner than the longer waves (red). This property of the lens is known as *chromatic aberration*. According to Helmholtz, if the media of the eye possess the same dispersive power as water, the rays of violet light must be brought to a focus at about 0.434 mm. in front of that of the red rays. If an object is in focus when viewed with red light, it can be shown that the focus must be changed if the same object is illuminated by violet light.

Spherical aberration depends upon the fact that rays entering near the circumference of a lens are brought to a focus sooner than those entering near the center. In the case of the eye, spherical aberration is avoided in part by the peculiar construction of the crystalline lens. "This organ is composed of concentric layers whose density increases toward the center. The result of this arrangement is that the center of the lens is more refractive than the periphery, and the tendency of the latter to refract more strongly is more or less neutralized." Furthermore the iris constitutes an adjustable diaphram which cuts off the rays that would otherwise go through the periphery.

This discussion of aberration is based on Howell's treatment in his Text-book of physiology, 1929, p. 321.

[2] C. S. Myers, Text-book of experimental psychology, 1922, Part I, p. 75.

not involve the actual creation of a border where no border exists in the physical stimulus. I have stumbled into the discovery of a phenomenon which clearly demonstrates this very thing. The phenomenon referred to is described below.

II. Border-Contrast with a Graded Stimulus

A glance at the pattern of the disc illustrated in Fig. 1 will show that if the disc were rotating fast enough to eliminate flicker, the distribution of physical intensity along the diam-

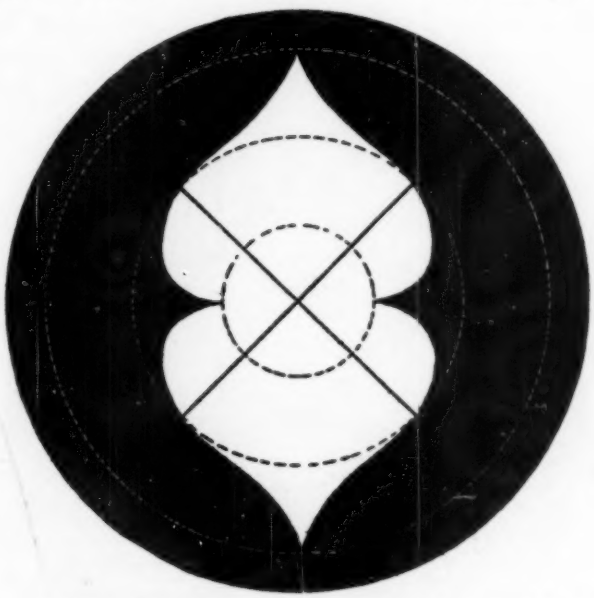

Fig. 1. A disc for demonstrating border-contrast with a graded stimulus.

eter [3] would be graded from the inner dotted circle to the outer dotted circle, and accordingly the appearance should be that of a blurred white patch on a black background with a zone of fusion between them. Instead, however, there appears on the black ground a sharply bounded, uniform, white disc, surrounded by a halo similar to the halo of an after image. The position of the border of the white disc is indicated by the middle circle in Fig. 1; this circle lies half way between the

[3] The distribution of physical intensity along the diameter is represented graphically in Fig. 6.

inner and outer circles, *i.e.*, at the mid-point of the graded zone. Here then is a graded stimulus which gives a visual image with a clearly defined border, and this indicates that border-contrast may involve creating a border where no border exists in the physical stimulus.

III. Physiological Processes Underlying Border-Contrast

According to Hering, the physiological processes underlying border-contrast are retinal processes, involving the interaction of retinal areas; *i.e.*, a "process of assimilation set up by a stimulus in one part of the retina brings about a process of

Fig. 2. A disc for demonstrating "graded contrast."

dissimilation of the same apparatus in neighboring parts of the same retina." [4]

According to Rollet and McDougall [5] the interaction takes place between the retino-cerebral elements; *i.e.*, contrast

[4] C. S. Myers, *op. cit.*, p. 89.

[5] Wm. McDougall, Some new observations in support of Thomas Young's theory of light- and colour-vision, *Mind*, 1901, **10**, p. 245.

is due "to the inhibitory action of a given cortical visual process upon the visual processes in neighboring regions. . . . McDougall supposes that such inhibition is due to drainage of nervous energy from the less active (or inactive) into the more active regions." [6]

The "phenomenon of graded contrast" [7] affords evidence for this theory. When the disc illustrated in Fig. 2 is rotated on a color rotator, a central light gray disc is seen surrounded

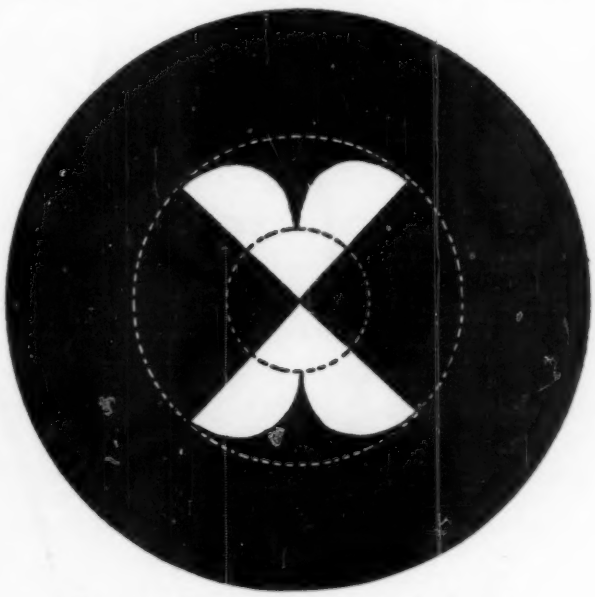

FIG. 3. A disc for demonstrating enhancement.

by a zone of darker gray, which gradually shades into the black background. Between the light gray center and the surrounding dark gray zone, occurs a narrow band which is brighter than the light gray center. To this phenomenon McDougall "has applied the hypothesis of 'drainage.' By virtue of their central connections he supposes that the more intensely stimulated cerebro-retinal elements drain away to

[6] C. S. Myers, *op. cit.*, p. 94.

[7] The 'phenomenon of graded contrast' was discovered by McDougall and is reported in *Proc. Physiol. Soc.*, March, 1903, vol. I., pages 19–21 in an article entitled Intensification of visual sensation by smoothly graded contrast.

themselves the energy from the less intensely stimulated; so that the final effect," [8] in a disc of the above pattern (Fig. 2) is to enhance the excitation effect where the bright band appears.

The hypothesis of 'drainage,' however, is not applicable to all phenomena involving graded stimuli, for when the disc illustrated in Fig. 3 is rotated on a color rotator, the patch of

FIG. 4.

When the disc demonstrating 'enhancement' is in rotation, the distribution of physical intensity along the diameter may be represented graphically by the curve in Fig. 5.

white appears uniform in spite of the fact that the distribution of physical intensity is graded from the inner dotted circle to the outer dotted circle. It seems that in this case the activity of the less intensely stimulated retino-cerebral elements is enhanced rather than inhibited; this, of course, is incompatible

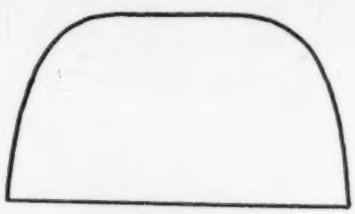

FIG. 5.

When the disc demonstrating border contrast is in rotation, the distribution of intensity along the diameter may be represented graphically by the curve in Fig. 6.

with the hypothesis of drainage. Both enhancement and inhibition involve perhaps the interaction of the retino-cerebral elements, but as to the nature of this interaction we have, it seems, no means of investigation.

[8] C. S. Myers, *op. cit.*, p. 96.

A glance at the disc which demonstrates border-contrast (Fig. 1) will show that it is a combination [9] of the discs demonstrating 'graded contrast' (Fig. 2) and 'enhancement' (Fig. 3).

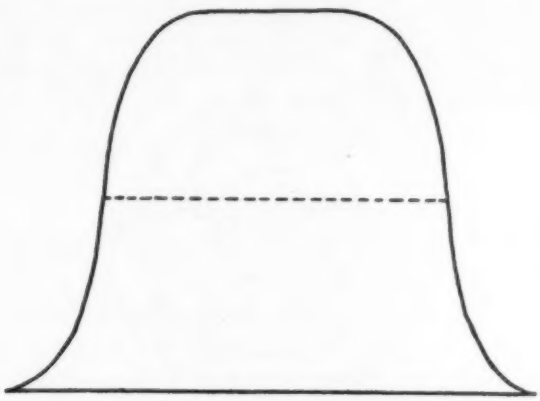

FIG. 6.

It is obvious that this curve is the result of compounding the two curves in Fig. 4 and Fig. 5.

IV. THE EFFECT OF IRRADIATION OF LIGHT UPON THE BORDER OF THE RETINAL IMAGE

It may be proved by the following theoretical consideration that the ill-defined border of the retinal image (which, according to Myers, is the inevitable result of the irradiation

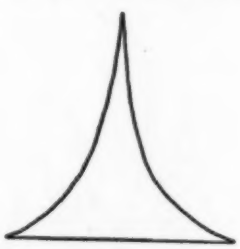

FIG. 7.

of light) is similar to the graded stimulus of the border-contrast phenomenon described above.

[9] When the disc demonstrating 'graded contrast' is in rotation, the distribution of physical intensity along the diameter of the disc may be represented graphically by the curve in Fig. 4.

A single luminous point, like the planet Jupiter,[10] cannot project its rays upon a single retinal point, but due to the inevitable irradiation of light the image spreads out over a circular area (which may be called the *diffusion circle*). We may well suppose that the light is concentrated at the center

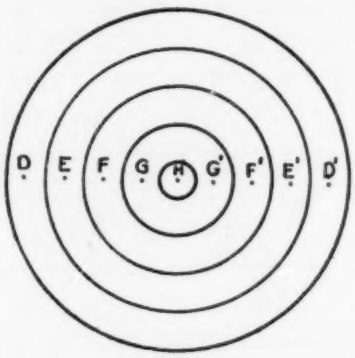

FIG. 8.

and grows less intense toward the edge in the manner indicated graphically in fig. 7.

If we exchange the single luminous point for a white disc, the retinal images of all the luminous points along the diameter of the white disc overlap; and the total distribution of intensity

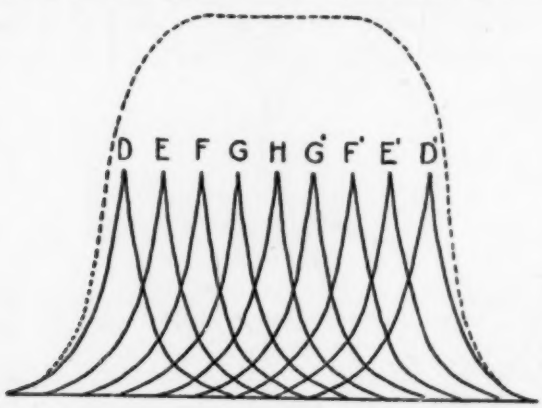

FIG. 9.

[10] The planet Jupiter seen with the naked eye appears as a tiny, self-luminous cloud with a hazy, ill-defined edge; whereas with telescopic magnification Jupiter appears to be a solid sphere with a well-defined edge.

along the diameter of the retinal image of the disc is the result of the summation of these overlapping images. Let points *D*, *E*, *F*, *G*, *H*, *G'*, *F'*, *E'*, and *D'* in Fig. 8 represent points along the diameter of the white disc and let all the light in the intervals between the concentric circles be regarded as concentrated at these points. The overlapping retinal images of these points are represented graphically in Fig. 9, and the dotted line which represents the total distribution of intensity is the effect of compounding these separate curves.

V. Conclusion

Since the distribution of intensity in fig. 9 (representing the ill-defined border of the retinal image of an external object) is similar to the distribution of intensity in Fig. 6 (representing the graded stimulus of the border-contrast phenomenon described in this paper) it seems that the border-contrast phenomenon confirms Myers' hypothesis that border-contrast may compensate for the ill-defined border of the retinal image which is the result of the inevitable irradiation of light.

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